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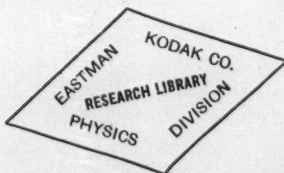
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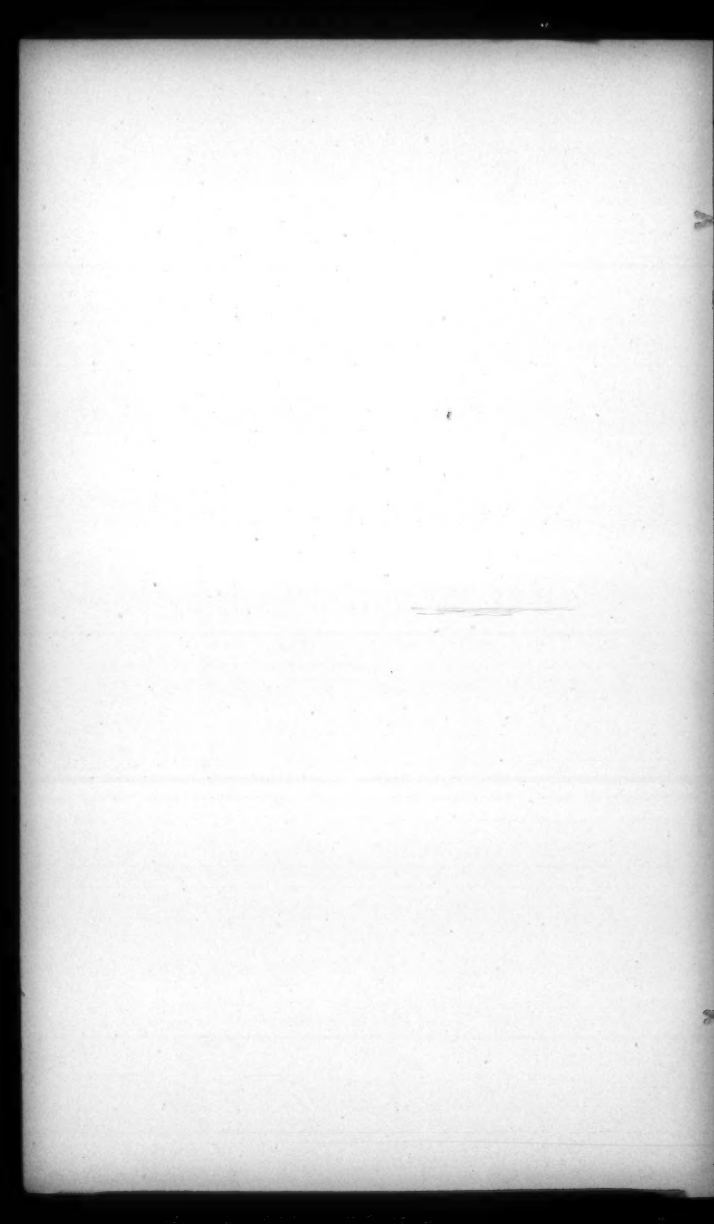
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THE THOMAS YOUNG ORATION, 1907.

THE DEVELOPMENT OF THE SCIENCE OF
PHYSIOLOGICAL OPTICS IN THE
NINETEENTH CENTURY.

BY PROFESSOR M. H. E. TSCHERNING,

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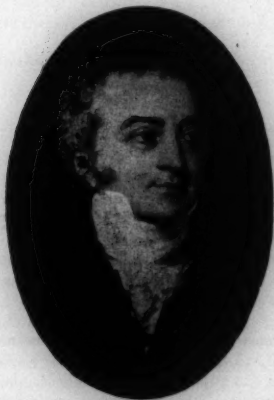
GENTLEMEN,

WHEN I received from the Optical Society the invitation to come to London to deliver the first Thomas Young Oration, I experienced much satisfaction. It gave me great pleasure to see that the man whom I consider one of the greatest geniuses who has ever seen the light in Great Britain, had at last received in his own country the honours due to him. It gave me pleasure to think that I had perhaps contributed a little to this result, and, above all, to think that the Optical Society had done me the great honour to choose *me* as the first lecturer. Now, I will first give you a short sketch of Young's life and his scientific activity, and then I will, in a few words, try to give you an idea of the development of some portions of the science of Physiological Optics during the last century, in order to show the influence of Young's ideas on posterity.

First, I would ask you to cast a glance on his portrait. It is a reproduction of the picture painted by Lawrence, that is now in the Royal Institution. By looking at this face no

one would be in doubt, I think, that it belongs to an Englishman, and I think that one would seek long before finding so fine an expression of the highest intelligence of Britain. This other portrait is a reproduction of the medallion in Westminster Abbey, executed by Chantrey, after Young's death. It seems to me that this latter does not express so well the characteristics of Young's face as the picture.

Thomas Young was born at Milverton, in Somersetshire, in a family that belonged to the



The Painting.

Society of Friends; he wore the dress peculiar to this sect, which, however, he abandoned later on. There are many stories of the extraordinary precocity of his mind. Tyndall relates that he could read very well when he was two years old, and that he had read the whole Bible twice, from the beginning to the end, before the age of four years. He seems to have never profited very much by regular teaching; he frequented

different schools, but wherever he went he surpassed not only his schoolfellows, but also his masters. He was sent to a school at Campton, in Dorsetshire, when he was nine years old, and stayed there nearly four years, but he seems to have worked there rather as a student at a university than as a schoolboy. The following little story will give you an idea of the astonishing nature of his mind. He interested himself in botany and saw that it would be necessary to him to have a microscope. Another would



The Medallion.

have bought a microscope; not so Young; he bought a treatise on optics and a lathe, and began to grind his lenses himself. But reading his treatise on optics he met with some fluxional symbols that put him in great perplexity, because he could not understand their meaning, and he was not satisfied until he had read a short introduction to the method of fluxions, in order to be able to understand the expressions in his treatise on optics. For a schoolboy of thirteen years, that is already a strange occupation, but where it grows really astonishing is when we hear that at the same time he learned, besides Latin and Greek, that were

part of the school teaching, five other languages—French, Italian, Hebrew, Persian, and Arabic. A rich gentleman, Mr. Barclay, at Youngsbury (Hertfordshire), wished to engage a private tutor for his grandson, and looked about for a proper companion of his studies; a common friend introduced Thomas Young to him. Mr. Barclay, on seeing this little Quaker, had not very great confidence in his capacity. In order to see if he wrote at least a good hand, he asked him to copy a phrase in a book. Young retired into another room, and remained there some time. When Mr. Barclay went to see what he was doing, he had translated the phrase into seven different languages. It is not surprising that with such a pupil the rôle of the tutor became illusive. Young was his own tutor at Youngsbury, as he had been at Campton. He stayed nearly five years at the house of Mr. Barclay, dividing his time between classical studies and higher mathematics, especially studying the optics and the Principia of Newton. In 1792 he left Youngsbury and came to London, where his uncle, Dr. Brocklesby, had a great medical position. Here Young began his medical studies, which he continued later on at Edinburgh, and at Göttingen, in Germany. After his return from the Continent he went to Cambridge, where he later on obtained the degrees of Bachelor of Physic and Doctor of Medicine. In 1797 Dr. Brocklesby died; he left him his house in London, his collections, and £10,000. In 1800 Young established himself as a physician in London; he continued to practise there almost his whole life; later on he practised also at Worthing, where he had a summer residence. But although he was appointed physician to St. George's Hospital he never had a great *clientèle*; his patients considered him as too learned. Like most young medical men in great towns, he had in the beginning very little to do; he therefore accepted the position of Professor of Natural

Philosophy at the Royal Institution, and gave lectures there during two years. But he was not at all qualified to be a popular lecturer; the lectures were much too difficult for the pupils. He later on published them; they form two immense volumes in 4to, treating of almost all questions of physics. It is true that they are not easy to understand for a student, but it is said of them that no writer on any branch of science can safely neglect them, so rich is the mine of knowledge which they contain. In 1802 Young was appointed Foreign Secretary of the Royal Society. Later on, the Secretary's office was offered to him, but he refused it. In 1818 he was appointed Secretary of the Board of Longitude, and abandoned then the greater part of his medical practice. He died in 1829, aged only 56 years.

Such were the circumstances attending the life of Thomas Young. But what gives interest to the study of his life is his extraordinary scientific activity. I cannot treat here all the subjects on which he worked. First, it would take too much time, and then he worked in so many different fields that it is impossible for *one* man to follow him in them all. But I will mention the principal among them. First, we have his celebrated paper *On the Mechanism of the Eye* (1801), to which I shall come back later on. In calling it celebrated I would not say that many know it. Few have read it, and fewer still have understood it; nevertheless, it forms with other works of Thomas Young the true basis of the science of physiological optics. Then we have the three papers on "Sound and Light" (1800-1802), in which he abandoned the Newtonian theory of light as produced by an emanation of corpuscles from the lighted body. He accepted the more ancient theory of Huyghens, that considers light as produced by vibrations in the ether, applying to it his celebrated principle of interferences, which should be the key for understanding almost all the phe-

nomena of physical optics. He stood quite alone with his ideas, and the Newtonians were contemptuous. One of them, who later played a great political rôle, Lord Brougham, wrote in the justly celebrated 'Edinburgh Review' a critique, in which he tried to ridicule Young. The latter wrote an answer that was quite crushing, but, having no journal at his disposition, he published it as a pamphlet. It is not possible to fight with pamphlets against a great journal; the answer produced no effect at all; the bookseller only sold one copy of it. The result was that Young did not write a word on optics for many years, except some reviews of papers by other authors. He then occupied himself with other parts of physics; he wrote his paper on cohesion of fluids; his hydraulic investigations, some medical papers, and he published his lectures of which I have already spoken. He wrote on the construction of ships, on atmospheric refraction, on an algebraic expression of the value of life, on tables of mortality, and many other subjects, showing everywhere at the same time his astonishing mass of knowledge and his deep originality.

Then it happened, in 1815, that a young French engineer, Fresnel, sent to the Academy of Science at Paris a paper on diffraction, in which, independently of Young, he had applied the theory of interferences. On reading it the French physicist Arago remembered Young's papers and showed them to Fresnel. It was rather depressing to him to see that he was not the first who had found the new principle, but he recognised without any restriction the priority of Young. There followed now in the next few years a very vehement discussion on the theory of light in the Academy. The frequent references to Young's writings called attention to him, and he began to be famous. This encouraged him to return to the study of optics, and he wrote in 1817 the article *Chromatics*, for the "Encyclopædia Britannica," in

which he made a very important addition to his theory. The partisans of the undulatory theory had in the beginning thought that the nature of light was quite comparable to the nature of sound. But in 1809 Malus had discovered the polarisation of light; under certain circumstances a ray of light has not the same properties in all directions perpendicular to its own direction. To explain this Young emitted the hypothesis, that the molecules of ether do not vibrate in the direction of the propagation of light—as the molecules of air *do* in propagating sound—but in a direction perpendicular to it. Fresnel came soon to the same conclusion, which is now generally accepted. In the following years Young did not work much on optics; he left to Fresnel the glory of building the whole monumental edifice of physical optics, of which he had himself laid down the foundation. The two men were good friends. In 1827 Young had, in his capacity of Foreign Secretary to the Royal Society, to forward to Fresnel the Copley medal which the Council of the Society had voted to him. The medal reached Fresnel on his death-bed. The same letter that announced the death of Fresnel to Young gave him intelligence of his own election as one of the eight foreign members of the French Institute, one of the greatest scientific honours a man can obtain.

One might consider it rather surprising that Young abandoned the development of modern optics to Fresnel. It was because he had so many original ideas that he would not sacrifice himself to one alone of them. In the course of little more than a year he wrote for the "Encyclopædia Britannica," besides the article on *Chromatics*, also the articles on *Bridge*, *Carpentry*, *Cohesion*, and *Egypt*, all of them remarkable for laborious and original research. The most celebrated of them is the article on *Egypt*, "The greatest effort of scholarship and ingenuity of which modern literature can boast"

('Edinburgh Review'). The principal object of the paper is the celebrated Rosetta stone, that is now in the British Museum. It bears the same inscription in three different scriptures, in Greek, in Enochial scripture, and in Hieroglyphs. The first one was easily read, of the second some words had been deciphered with great difficulty, but no one had been able to understand a word of the third. Here Young showed his immense penetration. He found out that certain signs, surrounded by an elliptical ring, which repeat themselves several times in the hieroglyphical inscription, correspond to the name of *Ptolemy* in the Greek inscription. In this manner he obtained the first letters of the hieroglyphical alphabet. These helped him to find others, and so he laid down the basis of modern Egyptology.

I hope that these brief notes have given you an idea of the extraordinary extent of Young's mind. He himself tells that during his last visit to Paris he went to the Institute one Monday, and had great pleasure in hearing the members of the Academy of Science discussing some theories of optics, based on his own investigations; then he went there on another day and heard the Academy of Inscriptions discussing some of his hieroglyphical discoveries. And although it did not happen, it might very well have happened that he should have heard a discussion on his theory of life assurance in the Academy of Political Science, or a discussion of his theory of music in the Academie des Beaux Arts, or even a discussion about his studies on the heart and the arteries in the Academy of Medicine. I think that the world has never seen anything like that.

From all I have said you probably fancy Young a stern meditative man, always confined in his study, when he was not occupied with his patients. Nothing could be more false. He was a man of the world, frequented the best society in London, and was highly appreciated

there. Arago states that of all existing instruments there were only two he would not play. When young, he was very fond of dancing, and in horsemanship he pushed his cleverness so far that one would hardly think it possible. As the result of a wager he learned at Göttingen to stay on the back of two horses running at full speed in the ring. His inheritance from his uncle, as well as his medical practice and his appointments, permitted him to live in a good style. His marriage seems to have been very happy, although he had no children. But his scientific activity gave him rather little satisfaction, partly through his own fault. He had the idea, for which he was indebted to his Quaker education, that *every man was able to do what every other man had done*, believing that there is in reality very little difference between the intellectual gifts of different men. One might fancy that this idea had guided him when he wrote his papers, and that he measured the intelligence of other people with his own, for he used the greatest conciseness, and his expressions are often so obscure that more than one of his readers has used the word "prophecies" in speaking about his papers. Young compared himself to a personage of Greek mythology (Cassandra), to whom a god had given the gift of foretelling the future, but added that no one should believe in her predictions. His scientific works gave him no satisfaction at all, until Arago and Fresnel discovered him, and he never had the popularity he ought to have enjoyed. Yet to-day his reputation seems greater on the Continent than in this country. As Arago said, when he died, Westminster Abbey had no place for him; he was buried in Farnborough, in Kent.

To finish this little sketch of Young's life I will ask your permission to use a parable in order to point out the particular position that Young occupies in the story of science. I will compare all people who work at any definite

science with a crowd of men, who are going on a broad road through a wild forest. The road is the result of many generations' work, it conducts to a beautiful point of view; on both sides is the wild forest where no one can go. But in the crowd of men who are going along the road, there is one whose mind differs from the others; he gazes the whole time into the forest, and suddenly says: "It seems to me that there should be a path there." The others shake their heads and say: "How foolish he is, that he cannot keep to the road as we others do." He goes into the forest, and he finds a path that leads him to another beautiful point of view. He comes back and tells the others of the path, but no one will follow him; they all continue to go on the old way and he, himself disappointed, speaks no more about it, and begins perhaps at last to doubt if there really was such a path. But in the memory of people lives the tale of the foolish man, who said that he had found a new path there. Then, some time long after, there comes a second man. His mind is larger than that of other men, but it does not resemble that of the first one. When he hears the tale, he says: "Well, if someone has been able to go there, other people also should be able to go." He enters the forest, and he finds the path and the new point of view. And he works, often his whole life, in order to make a broad road on which all people can travel. And the whole world proclaims him a great man, but only few think of the first one. Now, in almost all branches of science you can find examples of that sort, but what is surprising is that Young played the rôle of the first man in so many different branches of science. For if you take Young as the first man in the question of theory of light, the name of the second man is Fresnel; in the question of the anomalies of refraction of the human eye the name of the second man is Donders; in the question of colour sense you can call the second man

Clerk Maxwell, or Helmholtz; in the question of hieroglyphics the name of the second man is Champollion; in the question of terrestrial radiant heat the name of the second man is Wells, and I have not yet finished the list. For his own reputation it would certainly have been better if Young had completely developed but one of his ideas. But for the advancement of science it was better that he did as he did. For if the second man is not frequently met with, the first one is extremely rare.

I will now try to give you a little sketch of the development of some points of the science of physiological optics. Although the idea of comparing the eye to the camera-obscura is as old as that instrument itself, yet the first true ideas on the functions of the eye were only emitted by *Kepler*, in the beginning of the 17th century; he showed that the rays that fall on the eye are refracted by the cornea and the crystalline lens in order to form a little inverted image of the outer world on the retina. *Scheiner* demonstrated the image on a dead eye and imagined the experiment that goes under his name. In the 17th century we have also the discovery of the blind spot by *Mariotte*. In the beginning of the 18th century we meet the measurements of the visual acuity by *Robert Hooke*, the works of *Newton* that touch on our science in more than one point, and later on *Bouguer's* researches on light sense.

Much good work has been done by these authors, yet most of them seem now somewhat old. But reading Young's paper *on the mechanism of the eye*, we are, with one step, in the modern world of science. This is why we, in France, often call Young the father of physiological optics. I have translated this article with some other parts of Young's works touching physiological optics. I have added *Arago's Eloge de Young*, and a certain number of notes necessitated by the obscurity of Young's expressions. The whole form a volume of 278 pages,

which I have the honour to offer to the Society.

Although there are many other things to be found in his works, we can say that Young has in physiological optics three great merits. He first discovered the fault of the human eye that is now known under the name of astigmatism; he proved that the accommodation of the human eye for short distances is produced by an increase of the curvature of the surfaces of the crystalline lens, and he showed that all our colour sensations can be considered as composed of three simple sensations, *red*, *green* and *violet*, and that colour-blind people have only the two last.

In order to determine the refraction of the eye Young used his optometer. As I could not find this marvellous little instrument anywhere I have had it constructed again, modernising it a little. You see here the instrument; it is based on the principle of Scheiner's experiment. Dividing the pupil in two by placing a narrow band before it, one sees a luminous point doubled, except when the point is placed at the distance for which the eye is focused. Looking along a straight line one sees two lines intersecting each other at the focus of the eye. In order that the instrument should not be too large the eye looks through a convex lens of 10 D. With this instrument Young determined the refraction of his eye very prettily. He had a myopia of 4 D., combined with an astigmatism of 1.75 D. against the rule. The observation of a luminous point at different distances confirmed these measures. Excluding the action of the cornea by putting the eye under water, he showed that the fault was not due to any irregularity of the corneal curvature, as we now know often happens with inverse astigmatism. Young proposed to correct the fault by giving an inclination to a spherical glass. The man who first used a cylindrical glass to correct his astigmatism was the astrono-

mer *Airy*. In the following fifty years only very few occupied themselves with this question, until *Helmholtz* constructed his ophthalmometer for measuring the radius of the cornea. Working with this instrument *Donders* and *Knapp* discovered corneal astigmatism, and from that time cylindrical glasses came into vogue. At first the methods for discovering astigmatism were rather defective. *Donders* used the luminous point to find the direction of the axis, and the stenopaic slit and spherical glasses for determining the degree of astigmatism. *Javal* introduced the star figure and the direct measurement with cylindrical glasses, and, many years later, in collaboration with *Schiötz*, he converted the ophthalmometer into the clinical instrument that you all know. At the same time *Cuignet* invented the shadow test, which is also of great use for the objective determination of astigmatism.

Young's paper on the mechanism of the eye was especially written to prove that the accommodation of the eye for short distances is produced by an increase of the curvature of the lens. It did not produce the slightest effect. So late as 1851 *Arlt*, having discovered that myopia is due to an exaggerated length of the eye, concluded that accommodation is also produced by a lengthening of the eye, myopia being a result of working at short distances. The solution of the problem came about in another way. In 1819 *Purkinje* discovered the images produced by reflection on the surfaces of the crystalline lens. Many years after (1853) a young Dutch surgeon, *Cramer*, observed that the image, due to the anterior surface, grows small and approaches the axis, when the eye accommodates itself for short distances. He concluded very exactly that the surface grows more curved. *Helmholtz* made, independently of *Cramer*, the same discovery, and stated that also the posterior surface grows more curved, and that the thickness of the lens increases a little. He cal-

culated that the change is sufficient to explain the whole range of accommodation.

It remained to find out how the change takes place. *Sir William Bowman*, in England, and *Brücke*, in Germany, almost at the same time (1848) found the ciliary muscle, and it was almost sure that the accommodation must be due to the contraction of this muscle, the only internal muscle of the eye beside the muscles of the iris. Helmholtz fancied then that the crystalline lens, in the state of rest, is flattened by a traction exercised by the zonula, while the contraction of the ciliary muscle relaxes this ligament in order to allow the lens to take its form of equilibrium, that corresponds to the highest degree of accommodation. The explanation seemed strange, and was not adopted at once, but in proportion as the authority of the inventor of the ophthalmoscope increased, the belief in the theory grew stronger, and although Helmholtz had projected it as a mere supposition, it had, at the time when I began my ophthalmological studies, become a dogma. In the whole ophthalmological world only one man (Professor Schön) endeavoured to throw doubt on it.

In the list of authors whom I have cited you will hardly have heard a French name. In truth, there was very little interest in France for questions of that kind. Later on this changed. In 1867 Javal translated Helmholtz's admirable text-book on physiological optics, and in 1876 the ophthalmological laboratory at the Sorbonne was founded for him. In 1884 I became his assistant. One day we were speaking of the scientific value of men who had occupied themselves with physiological optics, and I expressed my admiration for Helmholtz. Javal said: "Yes, Helmholtz is a very great man. But I think that if you put five or six good physicists together, they would be able to do what Helmholtz has done. But you can put a hundred of them together and they would

not be able to do what Thomas Young has done." It seemed almost a heresy to me, but I undertook to read Young's paper *on the Mechanism of the Eye*. It was not easy at all to understand, but when I had finished the lecture I was much astonished. For I saw that not only Thomas Young, fifty years before, had made the discovery for which we all praised Cramer and Helmholtz, but also that he knew much more about accommodation than we all did eighty years later.

If you have a photographic apparatus focused for infinity and you focus it for a short distance, you are obliged either to lengthen the apparatus or to change the objective for a stronger one. By means of experiments so subtle that no one has ever been able to repeat them, Young showed that the length of the axis of the eye does not change during accommodation. Another series of experiments showed that the cornea does not change either. The most decisive of these experiments was to put the eye under water; he found that the power of accommodation was not diminished, although the action of the cornea was excluded. The only explanation possible, then, was that the curvature of the surfaces of the lens became increased. He examined some young subjects who had been operated for cataract, and found that they had completely lost the power of accommodation. Finally, he describes some observations which, as he says, come very near to being a mathematical demonstration of his solution of the problem. These observations show that the accommodation is much stronger in the centre of the pupil than near to the margin, a circumstance which proves that the surfaces of the lens flatten near the margin, at the same time as the curvature increases in the centre. Young endeavoured even to calculate the conical form the surfaces must take under the accommodation. This was to me the decisive point, for according to Helmholtz's theory the

lens should approach to the spherical form, and the accommodation should be stronger towards the margin of the pupil for the reason of the increased spherical aberration. I therefore undertook to verify Young's experiments, and I fully succeeded. One of them consists simply in observing the shape that a distant luminous point presents during accommodation. It takes the form of a luminous ring, surrounding a darker disc; if the refraction had been the same in all parts of the pupil the whole spot would have been uniformly illuminated. You see here a little instrument, the aberroscope. It is an ordinary plano-convex lens; on the plane surface is engraved a series of horizontal and vertical lines. Looking through that lens at a distant luminous point you see it is a luminous disc, crossed by the shadows of the lines. Now, if you do not accommodate, the shadows are straight, or slightly convex towards the centre; under the accommodation they grow concave towards the centre. The optometer enables us to measure the range of the accommodation. Thus, if you use two narrow slits, placed close together, you will find that the range of accommodation is much larger if you place them to correspond with the centre of the pupil, than if you place them close to the border. All these experiments require a young observer with a large (or dilated) pupil. I then constructed an instrument, the ophthalmophakometer, with which I measured the curvature of the surfaces of the lens. I succeeded in measuring the increase of the curvature in the middle of the surfaces and also the peripheral flattening of the anterior surface; indirectly I concluded that the posterior surface must also grow more flat towards the margin. My results have since been confirmed by *Jackson*, *Grossman*, and others. *V. Pflugk* has recently succeeded in fixing the dead lens in its accommodative shape; he has found that the posterior surface grows often a little concave in its peripheral parts. This

concavity increases during the accommodation. One of my pupils, Dr. Zeeman, has since observed this concavity in the living eye.

After all these experiments one would imagine that Helmholtz's theory was done for. But the same misfortune that happened to Young during his life seems to follow him after his death. As Newton's great authority cramped him in his life, so Helmholtz's authority hindered the spread of his ideas after his death. The great name of his author has given a longer life to Helmholtz's theory of accommodation than it otherwise would have had. But I think, for my part, that in a few years it will only offer a historical interest. When we exercise a traction on the zonula of a dead eye, we obtain a change in the shape of the crystalline lens that resembles very much what we observe during the accommodation in life. It is, therefore, likely that the contraction of the ciliary muscle exercises a traction on the zonula that produces the accommodative change of the anterior surface of the lens. At the same time the posterior part of the ciliary body and the peripheral part of the vitreous that adheres intimately to it, is drawn a little forwards in order to produce the change in the posterior surface.

Young's ideas on colour-sense have an importance equal to that of his other conceptions. *Clerk Maxwell* gave the experimental proof that we can really choose three spectral colours so that we can reproduce all existing colours, exactly, or almost exactly, by mixing these three. The base for Young's theory is that we can reproduce exactly the yellow colour by a mixture of red and green. Lord Rayleigh constructed an instrument showing two fields, the one lighted by yellow light, the other by a mixture of red and green, in variable proportion. Asking several people to make the mixture similar to the yellow, by varying the proportion of red and green, he found that there is a class of men who put so much green in the mixture that it

seems almost pure green to a normal eye. Such people are not to be confounded with the true colour-blind.

On the Continent Young's theory was quite unknown until Helmholtz adopted it. It is there generally known under the name of the Young-Helmholtz theory. On one point, however, Helmholtz differed from Young. This latter author admitted that colour-blind people are lacking the red sensation. But Seebeck had shown, later on, that there are two classes of colour-blind people. If you show them a spectrum, some of them will see the red part of it quite dark, others seem to see it almost with the same brightness as those with normal sight. Helmholtz considered the last ones as green-blind, but my researches seem to show that they are red-blind as the others. A normal eye lacking the red perception would be what Helmholtz called red-blind, an eye which has the fault that Lord Rayleigh has discovered, would, lacking the red, be what Helmholtz called green-blind.

Under Helmholtz's protection the theory very soon grew popular. But it seemed impossible that one of Young's conceptions should be generally admitted without opposition. A German author, *Hering*, imagined another theory according to which our chromatic system should not be trichromatic, but tetrachromatic, the four principal colours being red, yellow, green, and blue. This theory soon obtained many partisans, but it does not give a satisfactory explanation of the facts, and I think that the time is not far distant when it will be consigned to that part of paradise where dead theories still live on, joining there Newton's theory of emanation of light and Helmholtz's theory of accommodation, and so many other conceptions which have been in opposition to Young's ideas. There is a word of Kepler *nihil dat, quod nihil est*—it is nothing, for it gives nothing—that one could apply to hypotheses. I am not aware that Hering's hypothesis has given us anything.

Young's hypothesis has led to the discovery of Lord Rayleigh's anomaly and to the nice method of reproducing colours, first proposed by *Cros* and *Ducos du Hauran*, and recently used for the autochromatic photographic plates. I have the honour to present to you some photographs taken with these plates, that the house *Lumière* has kindly placed at my disposition. The layer of gelatine is covered with a layer of starch cells, coloured with the three principal colours, red, green, and violet, carefully mixed together. If, for example, you form the image of a red flower on the plate, the red rays will reach the gelatine wherever there is a red starch cell, while they will be intercepted by the green and the violet cells. By developing the plate, silver will



Fig. 1.

be precipitated that covers all the red cells; the plate is at that moment negative; by looking through it towards the sky, the flower is seen bluish-green, the green and violet rays going through the plate, and the red ones being intercepted. In order to make the plate positive you dissolve the silver, expose the plate to the daylight and develop anew. Then the metallic silver covers the green and violet cells, while the red ones are free. The flower is seen in its natural colours.

As the colour-blind has only two of the three simple sensations, green and violet (blue), if we had a lamp that only emitted green and blue light there would be no difference between

colour-blind and normal people in a room only lighted with that light. You see here a mercury lamp. It does not completely fulfil the condition; it contains none, or very little red light, but there is much yellow light in it. Yet I think it can give some idea of the vision of colour-blind people. But the mercury lamp is also interesting for another reason. By looking at the lamp, especially by interposing a blue glass, you will see the whole field filled with a crowd of small luminous bodies that appear, move a little in an irregular way, and then disappear, to be replaced by others. You can also see the phenomenon by looking at the sky, but it is much more beautiful with the mercury lamp.

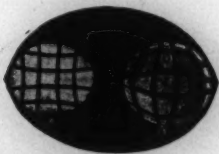


Fig. 2.

There are reasons for thinking that this phenomenon is due to the blood corpuscles in the retinal vessels, although it is not easy to say in what manner. Professor Barrett thinks they are due to the white corpuscles, and I think he is probably correct. They might very well act as small globular lenses concentrating the light on the layer behind the vessels. Now, I have noticed the very remarkable fact, that if you separate the light in its components, the phenomenon is extremely pronounced in the blue light; in the green and the yellow you only see traces of it, and in a red field produced with the sunlight you do not see anything of it at all. It seems, therefore, that the blue colour is perceived by a layer of the retina, situated nearer

to the blood-vessels than the layers perceiving other colours. According to Young's theory we should, indeed, expect different terminal organs for the three simple sensations.

After this little sketch I think you will have seen how Thomas Young's ideas have filled the whole century with struggle, and how we have, at many points, finished by coming back to his ideas. At Berlin and in Paris laboratories have been established for the study of the science which Thomas Young created, and it is with profound satisfaction that I hear that in his own country a similar laboratory has been at last instituted. And if there should be among you some young man who interests himself in this science, I would give him the advice that Javal gave to me, when I began my studies: "*Read Thomas Young.*"

[I take this opportunity to express my gratitude to my friend and pupil, Dr. Ettles, for his invariable kindness to me, whenever I have needed his assistance on this occasion.]

RESEARCHES ON POLISHED SURFACES.

BY G. T. BEILBY, F.R.S.

Lecture delivered on November 21st, 1907.

THE ideal polished surface is one which perfectly reflects a beam of light according to the well-known law of reflection; if any stray light leaves the surface at angles other than that of the incident beam, the polish is imperfect. A beam of light falling on the clean, undisturbed surface of a liquid is reflected in this way whether the liquid is transparent like water, or opaque like mercury. For the critical examination of a polished surface the specimen is placed on the stage of the microscope and an oblique beam of suitable intensity is focussed upon it by a condenser; all stray light is as far as possible excluded from the microscope. If the surface is clean, is free from structure, and is perfectly polished, no light ought to pass through the microscope to the eyepiece, and the field of vision should appear perfectly black. If the surface is imperfect, some rays will pass through the microscope to the eye and a picture of the surface may be obtained. It is hardly necessary to remind expert microscopists that the pictures produced in this way do not necessarily represent the true nature of the structure or irregularities of the surface; but at least they may be taken as a sure indication that some irregularity of the surface or a structure of some kind does exist. The delicacy of this test may be pressed to its extreme point by using an illuminant of great specific intensity. In some experiments which will be referred to later, a critical image of the sun was focussed on the centre of the field of vision on a polished surface which had been very slightly etched. By

this searching light it was found that surface irregularities of only a few molecules in thickness could be detected. Under this illumination the minute dust specks which are always present in the atmosphere of a room, can be watched as they alight on the surface like brilliantly luminous balls, sometimes rebounding from their first impact and then rolling swiftly over the surface before coming to rest. Indeed, these microscopic "fire balls" may become so distracting in certain states of the town atmosphere that it is necessary to exclude them by carefully screening the surface under examination.

While this method of illumination by means of a very intense oblique beam, is invaluable for the detection of surface structure or irregularities which are of ultra-microscopic dimensions, it requires to be used with caution when it is desired to follow the microscopic changes of structure which occur during polishing and other surface operations. For this purpose illumination by a reflected or transmitted beam, either strictly normal or slightly oblique, is most trustworthy, but when light of considerable obliquity has to be used, the most trustworthy results are obtained when it is first somewhat diffused by reflection from a white surface. In some cases it is advantageous to combine illumination by a unidirectional beam with a moderate illumination by diffused light. In this way it is possible to bring out, and, if need be, to exaggerate particular structural features by the judicious use of light and shadow. It is hardly necessary to observe that this kind of illumination generally involves the use of objectives of fairly large working distance. It ought further to be remarked that pictures obtained in this way should always be interpreted in conjunction with those obtained by other methods of illumination and with lenses of greater resolving power. To the expert microscopist of the older school it is a truism to say that no micro-structure can be confidently interpreted

till it has been viewed at various degrees of magnification and by different methods of illumination.

It is now seven or eight years since my attention was first arrested by the appearances of minute structure disclosed by the critical method of examination on surfaces which to other methods of examination appeared smooth and structureless. In some cases the surfaces lighted up brilliantly under the oblique beam and displayed in the microscope an appearance of regular structure to which the name of "spicular" was given. By the study of a variety of solid surfaces and of thin translucent films supported on glass, the true nature of the structure to which this "spicular" appearance is due was ascertained. By employing transparent or translucent films it was possible to apply illumination by transmitted light, so that lenses of the highest resolving power became available. In this way the "spicular" appearance by oblique light was definitely associated with a very fine and extremely shallow granular structure in the film. So long as this granular structure is not too shallow to be visible by transmitted light the "spicular" appearance can be produced by an oblique beam of very moderate intensity, and the "spicular" structure then corresponds very closely with the true structure as seen by transmitted light. When the granular structure becomes so shallow as to be barely visible even by slightly oblique transmitted light and a more intense oblique beam has, therefore, to be used to produce the "spicular" appearance, the diffraction discs of which the picture is built up become larger, and the pattern they depict departs further and further from the true structure.

I may remind you that the relation between the structure of a transparent body, as shown by these two methods of illumination, is familiarly seen when a diatom is examined respectively by transmitted light and by dark

ground illumination. Expert microscopists recognise that the latter method is less trustworthy when minute structural detail is being sought for. My own experience is that the method of dark ground illumination through the substage condenser is not so satisfactory as the direct method by an oblique beam thrown on the surface from above. The only advantage which may be claimed for the former method is that by it lenses of higher resolving power and smaller working distance can be used, but I cannot recall any instance in which any obvious gain resulted from this.

Throughout the lecture it will be necessary to refer to certain structural forms as "surface tension forms"; it is, therefore, desirable that I should explain more fully what is meant by this expression. I may remind you that surface tension in liquids is that property in virtue of which a liquid surface behaves as if it were a stretched elastic skin which completely encloses the mass of liquid within it. The behaviour of this skin has sometimes been likened to that of a thin sheet of india-rubber, and up to a certain point this analogy may, no doubt, be helpful, but it completely breaks down in one very important particular. The contractile force of the rubber sheet is not constant, but beginning at zero for the unstretched sheet, it increases as the surface is stretched. In the liquid skin on the other contrary the contractile force per unit area is constant, not only under all increases of its area by stretching, but also under all decreases by contraction down to the point at which the range of the molecular attraction is reached. As the area is increased by stretching, additional molecules pass from below the surface and take their places in the skin, and as it decreases, molecules pass from the skin into the interior where their attractive force is balanced by that of the surrounding molecules, and they become neutral so far as surface tension is concerned. This idea of an

almost indefinitely contractile skin, if once firmly grasped, supplies a clue which enables us to understand the varied and often strikingly-unlike forms which result from surface tension in liquids, and, as I hope to demonstrate to you, *in the more minute aggregation of solids also.*

In any moderately large mass of liquid the force of gravity keeps the surface horizontal

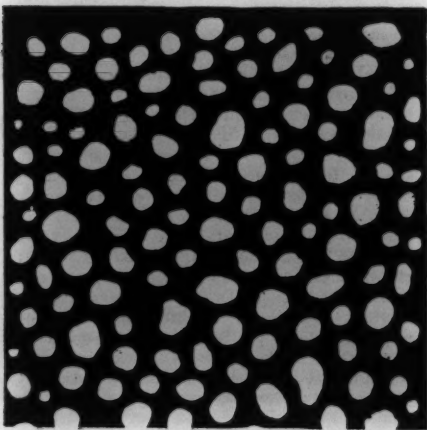


FIG. 1.

and flat, but when the force of gravity becomes small as compared with the contractile force of the surface skin, as it does when the mass of liquid is very small, the contractile force begins to overpower the gravitational force and the surface becomes convex. As the mass is further decreased the convexity of the surface increases till a point is reached at which the whole mass becomes spherical, this being the form in which

perfect equilibrium of the skin is reached. The soap bubble, the raindrop, the dewdrop, the lead-drop, and the mercury globule, all assume the spherical form under this influence. The effects of gravity and surface tension on small liquid masses can be very conveniently studied in the case of mercury, because mercury can lie on most surfaces without wetting them. If the mass of mercury is sufficiently small it is per-

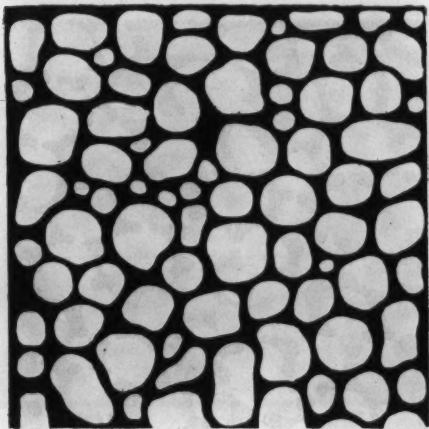


FIG. 2.

fectly spherical. As the mass is increased the side of the sphere which rests on the supporting surface is slightly flattened; as the mass is further increased the whole sphere is flattened, but the surface still remains convex, but a point is reached at which gravity so completely overpowers surface tension that a truly flat upper surface is developed.

When an oily liquid is allowed to spread on the surface of water a new element requires to

be considered. If the oily layer is of sufficient depth the force of gravity will be in the ascendant and the upper surface of the layer will be flat and parallel with the water-surface. But if the thickness of the oily layer is sufficiently reduced, the effect of gravity practically disappears, and surface tension becomes the main controlling force. But now it is not alone the surface tension of the oil which must be con-

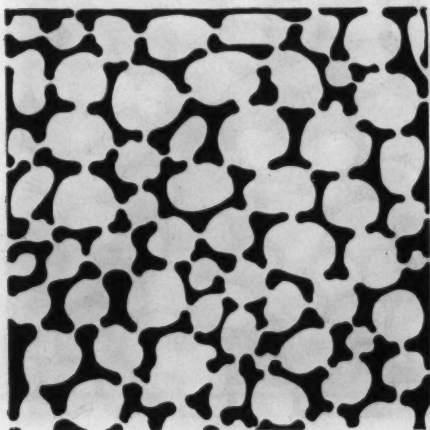


FIG. 3.

sidered, but also that of the water. The latter acts normally to the surface and its general tendency is, therefore, in the same direction as that of gravity, namely, to draw the oil over the water surface in a uniform layer. As the thickness of the oil film is reduced the equilibrium between the surface tension of the water and that of the oil, becomes unstable, and a state is reached in which the oil is partly pulled over the

surface of the water by the surface energy of the water, and partly drawn up into drop-like forms by its own contractile energy. This condition of instability lasts for a considerable time during which the forms assumed by the oil film are both interesting and beautiful. I hope to show you some of these changing forms by directly projecting the image of an actual film on the screen, but in case this experiment should

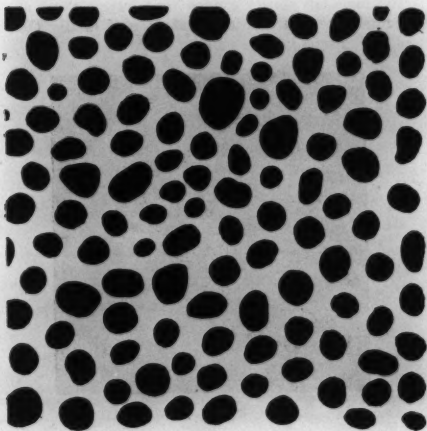


FIG. 4.

fail, the drawings on the wall have been prepared to show these forms in a somewhat diagrammatic way.

When the centre of the water surface is touched with a needle which has been dipped in an oily liquid, a film shoots out over the surface in all directions. At first this film appears quite continuous, but presently it becomes punctured all over with small round holes caused by the retraction of the oil under the contractile

force of its own skin. As the retraction proceeds the holes become larger and the film between them becomes thicker. The larger holes as they extend their area swallow up the smaller holes which they encounter in their extension. As the holes enlarge, the spaces between them become mere connecting ridges. Presently, the ridges show signs of irregular thickening at certain places and of thinning at others. This continues till the thick portions have grown at the expense of the thinner parts, and finally the



FIG. 5.

ridge divides at a thin place by the drawing up of the oil into the thickened parts on either side. The separated portions assume the form of rods with rounded ends which gradually approach more and more to the circular form. This is a very much simplified history of what actually occurs, but if the changes are narrowly watched with a magnifying glass they are seen to be complicated by the fact that on every portion of the film surface there are changes proceeding which are a repetition, on a much smaller scale, of

those occurring on the film as a whole. The recognition of this feature, the repetition of these changes of structure on a smaller and smaller scale, is of importance, as it supplies a valuable clue to the origin of certain structural forms which are to be found stereotyped in films produced by the direct aggregation of solids. The wall diagrams, figs. 1, 2, 3, 4, show four steps in the film changes; first, the perforation of the film by small holes; second, the enlargement of these holes and the gathering up of the film into mere ridges between them; third, the

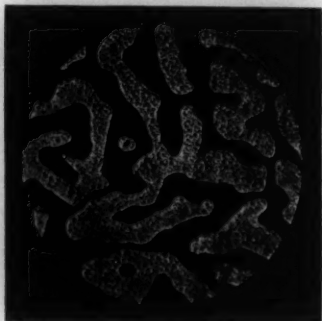


FIG. 6.

dividing of the ridges into separate pieces, and fourth, the final state in which the oil is gathered into isolated discs, or flattened droplets.

By allowing thin films of a solution of eosin in water, to which a little gum had been added, to dry slowly on glass plates, most of the steps in the passage of the continuous film to the final stage, when the film has collected in discs or flattened droplets, can be stereotyped in a convenient form for reproduction as photo micrographs. Two specimens of these are shown. In

the first slide, fig. 5, 6, the openings in the film are of all sizes and shapes from small round perforations to the large openings surrounded by ridges. These ridges also show thick and thin portions, and some of them have already divided, and the separated portions have drawn themselves into more or less circular forms. At the sides of the photograph the film is granular and unperforated. This granular texture is here seen to be a stage which precedes the actual perforation. The granulation is caused by the heaping up of the substance in

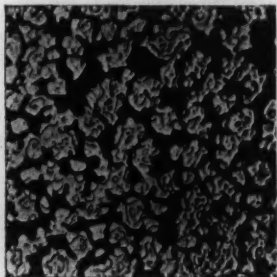


FIG. 7. × 440.

tiny ridges round central depressions. The photograph was made by transmitted light, so that the bright spots are thin places, and, therefore, depressions in the film. A number of these depressions nearer the centre of the plate have actually perforated, showing the clear glass below. This granular film structure is of peculiar interest, and will be referred to later when the structure of thin metal films is being considered. The ridges round the large holes also show the granular structure. Their presence illustrates what was said as to the repetition of

smaller and smaller forms on the larger elements of structure.

It is to be noticed that though the greater part of the substance has been drawn up into the bolder forms, yet a thin transparent film still remains on the surface, and in this the structure is repeated on a smaller scale.

The various forms assumed by liquid films under the influence of their contractile skin are thus seen to range from a minute pitting, or granulation, which can be of great uniformity even over a large surface, to the bolder open-work patterns in which the liquid has been

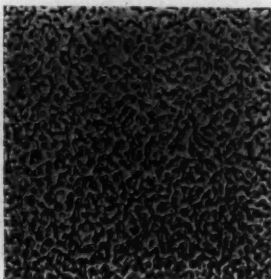


FIG. 8. $\times 440$.

drawn up into comparatively massive ridges. Having thus become familiar with the surface tension forms which are met with in thin mobile liquid films, we are prepared to pass on a study of the corresponding forms which have been discovered in thin films of metal in the solid state.

It is not necessary on this occasion to describe the varied methods by which the metallic films were deposited on glass or mica; for the present purpose it is sufficient to say that films of gold, silver, and platinum, were obtained in thick-

nesses which ranged from a few millionths of a millimeter up to several hundred times that amount. These films being firmly adherent to the glass or mica can be regarded as in a condition analogous to that of a thin oil film on the surface of water or to the eosin film on the glass surface, but with this fundamental difference, that while the water-oil combination was throughout in the mobile liquid condition, the metal-glass combination was supposed to be in a condition of solid immobility. The mobility which was actually found to have occurred in

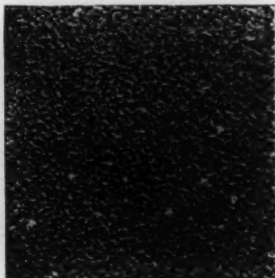


FIG. 9. $\times 440$.

the metal films was produced by heating them in an air bath to temperatures ranging from 250° to 400° . These temperatures are far below the melting points of the metals, as well as of the mica and glass supports.

The slides now shown are photographs, at an original magnification of 400/500 diameters, of gold films of three degrees of thickness. The first was about as thick as ordinary gold leaf, and before annealing showed the usual rich olive green translucence by transmitted light. The other two were considerably thinner than this,

and were blue, not green, by transmitted light. (Slide, gold films.) The thick film, fig. 7, after annealing, shows very plainly that surface tension has been at work, and we are, therefore, obliged to conclude that the molecules have been in a state of free movement under the influence of this force. The forms assumed by the gold are remarkably like those which were taken by the eosin solution, fig. 6, or like the more transient forms which were seen in the oil film on water (fig. 3). As with the eosin, so with the gold, there is a structure within a structure. The drawing up of the opaque and more massive figures has left a thinner film of gold on the

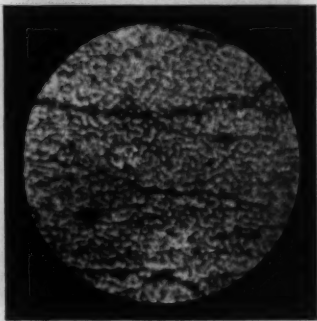


FIG. 10. $\times 1000$.

glass which in its turn has been drawn into perfectly transparent reproductions of the same type of figures on a rather smaller scale. In the next film, fig. 8, which was less massive, the opaque figures are much smaller and show that the final stage in aggregation under surface tension had been reached, the aggregates being mainly in the form of rods or rounded patches. The transparent part of the film is peculiarly

interesting on account of sinuous interlaced pattern which has been developed, no doubt, under the combined attractive force of the supporting glass surface on the one hand, and that of the more massive gold aggregates on the other. The third and thinnest film, fig. 9, is free from massive opaque aggregates, it was entirely transparent and completely covered the surface of the glass. As a whole it is very much more uniform and is much more typical of the thin film structure than the two first films are. It would be

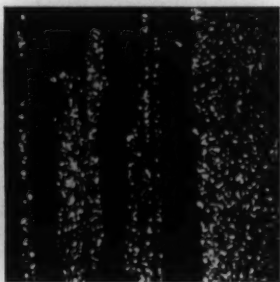


FIG. 11. x 580.

easy to multiply illustrations of this phenomenon to any extent, but only two more slides will be shown to further illustrate an important point.

At the outset it was stated that the "spicular" appearance had been traced to a certain structure which has an objective existence. The structure of even the thinnest of the gold films is sufficiently bold to show distinctly by transmitted light. The silver film now exhibited, fig. 10, though of very fine texture is sufficiently thick to show the granular structure and its surface tension origin very distinctly even

by transmitted light. By obliquely incident illumination the silver film, fig. 11, shows a structure which corresponds very closely with that of the previous photograph. While the more prominent mounds, or granules, which have caught the light appear as ill-defined discs, their more shallow neighbours are fair-

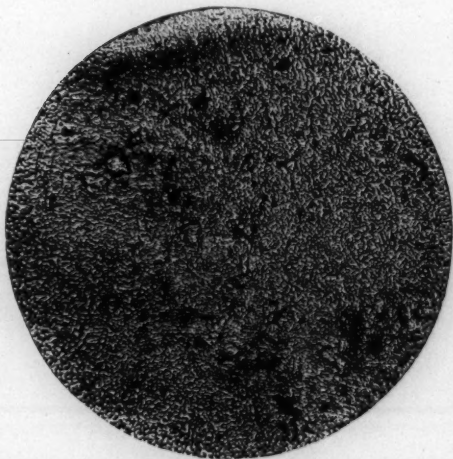


FIG. 12.

× 350.

ly well defined. In this photograph a line has been scraped across the film so as to show the torn edges against the dark glass surface. The continuity of the film over the whole surface is thus made clear; it is a film, not a disjointed collection of mounds. The tendency of oblique illumination to exaggerate the details of structure can be distinctly seen in this photograph.

The granules are made to appear as if their depth or thickness is approximately equal to their diameter, whereas measurements of the thickness of the film show that the granules must really be very slightly convex, their depth being only a small fraction of their diameter. It has already been pointed out that this exaggeration is of value when it is desired to push the critical

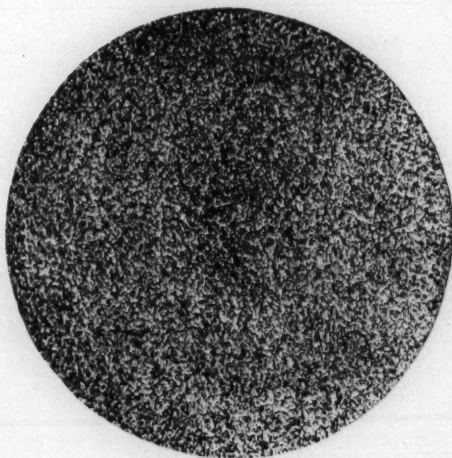


FIG. 13. $\times 350$.

examination of surfaces beyond the range of ordinary microscopic vision. The next slides are intended to illustrate this point. A very thin platinum film was photographed by the two methods of illumination, oblique and transmitted. By transmitted light the structure has been resolved by high magnification into the usual granular structure. A very intense

oblique beam was necessary to produce a photograph by a reasonably short exposure, the spicular appearance, though more brilliant, is obviously much less truthful than that obtained with the silver film. The last photograph of this series is of the polished face of a calc-spar crystal, which had been so slightly etched that by all ordinary methods of examination it

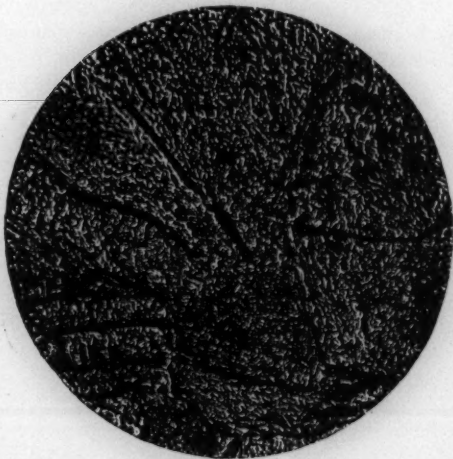


FIG. 14. $\times 350$.

showed no departure from a perfect polish. By illuminating the surface by a condensed beam of sunlight it was possible to obtain a photograph which shows the spicular appearance quite distinctly. From data which cannot conveniently be discussed on the present occasion it was calculated that the etching had only disturbed the surface to a depth of about one millionth of a millimeter, which is equal to about three times

the diameter of the molecule of calcium carbonate. Thus, having traced the minute surface structure to the last point at which it can be definitely resolved by the microscope, and having at the same time traced the association between the spicular appearance and this structure, we are justified in considering it as probable that the ultra-microscopic structure which

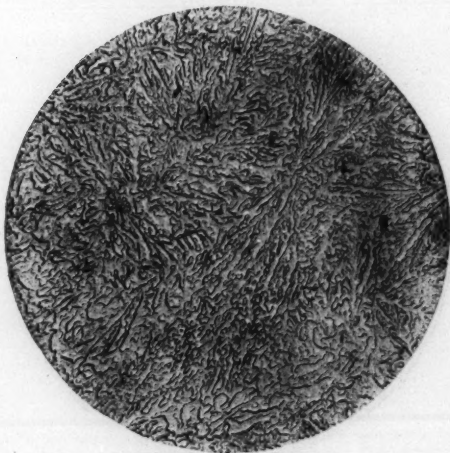


FIG. 15. $\times 350$.

is indicated by the spicular appearance is caused by the presence of the same type of structure, but on a smaller and smaller scale till molecular or nearly molecular dimensions are reached.

Another proposition has, I think, been fairly established, namely, that an increase in the mobility of the molecules of a solid which is still far short of the freedom of the liquid state, is sufficient to enable the force of surface tension

to assemble the molecules of a thin film into new forms of aggregation.

The molecular mobility which occurs at the moment of the partial solution of a solid surface, or on the separation of a solid from solution, are also occasions which may be seized by the forces of surface tension with the result that the forms of structure which are due to these forces are found both on surfaces which have been slightly etched by solvents, and in solid films which have been deposited from solution. At the surfaces of solids and of liquids alike,

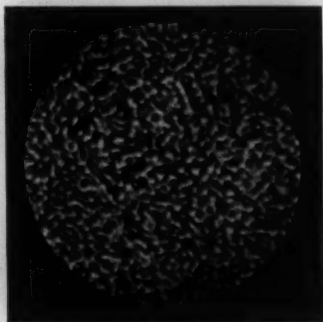


FIG. 16. GOLD. $\times 1000$.

these potential forces are always present and ready, as it were, to take advantage of any increase in the mobility of the molecules to impress their characteristic forms of aggregation upon them. To illustrate this point the behaviour of two very widely different substances will be referred to—gold and glass. The polished surface of a plate of pure gold was very lightly etched with chlorine water, so lightly, indeed, that the surface appeared to be only slightly dimmed where the solvent had acted; but

when examined by a lens of high resolving power under normally reflected light the originally smooth surface was seen to have given place to a fairly uniform structure of exactly the same type as that seen in the thinnest annealed gold film. The first slide shows the original smooth surface, and the second, fig. 12, shows the now familiar surface tension forms of one of the finer types. The fire-glazed surface of a glass slip was lightly etched by exposing it to the action of hydrofluoric acid gas mixed with air. As was the case with the etched gold,

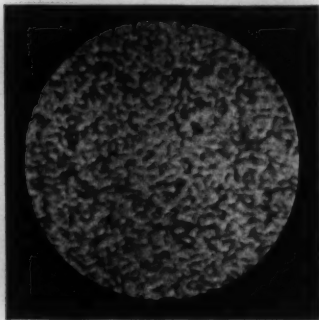


FIG. 17. GLASS. $\times 1000$.

the surface appeared to be only slightly dimmed by the action of the acid, but on examination by the same lens, and by transmitted light a surface structure was disclosed, so remarkably like that developed on the gold plate, that it would be very difficult to distinguish the two photographs if they were looked at apart, figs. 16, 17. By continuing the etching so as still further to remove the surface layers, the crystalline structure of the gold was disclosed, in this case it happens to be a broken and distorted

structure, as the plate had been prepared by hammering it out to four times its original area, fig. 14. The glass surface was more deeply etched, and in this case, also, a different structure was disclosed, fig. 15. Whether this is to be regarded as a surface tension form of structure, or as a crystalline form, or as a compromise between the two forms, cannot very well be discussed on the present occasion, but I am disposed to look upon it as a compromise. Microscopists are familiar with the compromises of

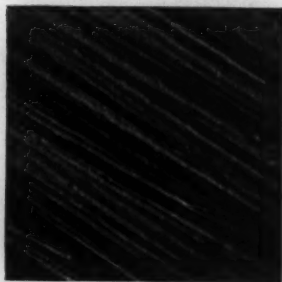


FIG. 18. $\times 1500$.

this description, which are seen when very thin films of salt solutions are allowed to crystallise on a glass slip. If the film of solid salt is below a certain thickness, the forms assumed are purely those due to surface tension. As the thickness of the film is increased, the crystalline force gradually overpowers the surface tension, and the deposit becomes more and more sharply crystalline. My own observations have shown that there is a definite limit for each substance below which the crystalline force is completely controlled by surface tension.

Having familiarised ourselves with the influence of surface tension on the aggregation of molecules of various degrees of mobility, we are now prepared to pass on to the consideration of the particular kind of mobility which is conferred on the molecules of a solid by various forms of mechanical disturbance.

A surface may be disturbed in various ways when a moving point, made of some material harder than itself, is passed over it. The sur-

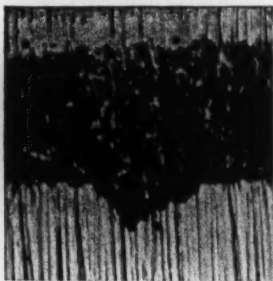


FIG. 19. × 1500.

face of the greater number of solids can be "scratched" by the point of a hardened steel needle, and the number of substances which cannot be scratched by a diamond point is extremely small. To the mineralogist the scratching test conveys a great deal of information by which he is enabled to classify mineral substances of every type. "Softness," combined either with tenacity or with friability, at one end of the scale, and the corresponding qualities associated with "hardness" at its other end, can provide a sufficiently extensive series of pigeon holes for the reception of every kind of mineral from metallic gold, or soap stone. at one end of

the scale, to the diamond, or corundum, at the other. Now, I am going to make an exceedingly strong statement, it is this: *In every one of these substances, if it is sufficiently homogeneous to be polished at all, the fundamental principles on which polishing depends are absolutely identical.* This may be supplemented by another equally strong statement: *The polished surface on a solid substance is as truly due to the presence of a surface tension skin as is the surface of a liquid.*



FIG. 20.

× 775.

The first illustrations by which I shall attempt to justify these propositions are probably familiar to some of my audience, for they were published some years ago and have been reproduced on various subsequent occasions. My excuse for producing them now is that their selection originally, when this theory of polishing was first put forward, has been justified by the fact that fuller experience and observation, while it has supplied many new illustrations, has produced none which are better fitted for their purpose.

When a soft metal like silver is filed, even a very casual inspection of the surface shows that

it has been ploughed into ridges and furrows by the projecting points of the file. If the file cuts are fine, there is nothing in the appearance of the surface to show that any abrupt breaks in the regular removal of the metal have occurred. But if a coarser file is used there is evidence of the tearing out of metal in irregular pieces and the ridges and furrows are far from smooth. The ploughing effect in soft metals is so familiar that it might readily pass without arousing any special interest or any idea that its existence called for a more intimate explanation. The

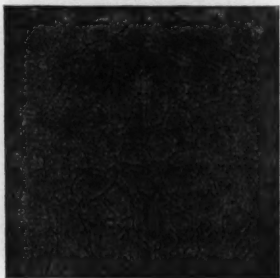


FIG. 21. $\times 775$.

convenient statement, that softness and toughness are intrinsic properties of these metals, and that ploughing and smearing are, therefore, a natural response to the mechanical treatment by tools or abrasives, does not necessarily strike us as unsatisfying.

But this as an explanation is bound to appear less and less satisfactory if the attempt is made to apply it to hard and brittle substances in which their quantities of softness and toughness are conspicuously absent.

Speculum metal gets its name from the fact that it was found to be a peculiarly suitable

material for the manufacture of mirrors for telescopes and other optical appliances. It is an alloy of copper and tin, and it is so hard and brittle that it cannot be worked by ordinary tools. In some states it is as brittle as glass and can easily be shattered by a blow with a hammer. Yet it is a remarkably easy substance to polish. Herschel, who had great experience in the grinding and polishing of mirrors for astronomical telescopes, believed that these two operations were identical, polishing being merely a finer kind of grinding. His idea was that the essential feature of polishing was the gradual flattening of the surface by the *removal of*

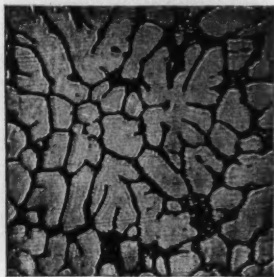


FIG. 22. × 775.

material. This is an exact description of the operation of grinding, but the essential feature of the polishing operation is left out. What this essential feature really is will be made clear by the next set of photographs.

The first slide, fig. 18, shows the furrows ploughed on speculum metal by the finest watch-maker's file. The photograph was made with an oil immersion lens of the highest resolving power and the magnification of the original negative was 1,500 diameters. Even on the

most sharply focussed portions, every part of the surface is smooth and rounded, there are no suggestions of crystal facets or angles. Even in the large furrows the varnish-like effect over the surface is very evident.

The second slide shows the effect of several successive polishings with the finest emery, the wide and irregular furrows left by the file have now given place to the much finer furrows left by the emery. The furrows now number about 150 to the millimeter.

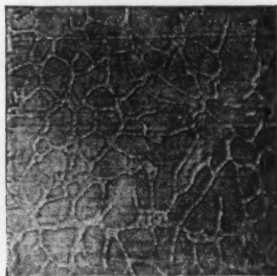


FIG. 23. $\times 775$.

The third slide, fig. 19, shows a surface of the same metal, across which a scratch had been drawn with the point of a very fine needle, the width of the scratch was about $1/75$ of a millimeter, or the $1/1800$ th of an inch. The surface was again polished on the same emery paper across the scratch. The scratch has acted as a trench in which the shavings ploughed out by the emery have been intercepted, and are seen in a heap, the top of which nearly reaches the surface level, and is in fairly good focus. It is seen that these "shavings" are really rounded bodies which are much more like drops than

like shavings or turnings. Their appearance is a further incidental proof of the flow which accompanies the ploughing action.

Leaving this side issue and returning to the simply furrowed surface, the fourth slide, fig. 20, shows the effect of polishing on rouged leather across the line of the emery furrows. This is somewhat striking. The extremely fine particles of rouge appear to have gripped the surface layer all over simultaneously, and to have flowed and dragged it across the emery furrows partly filling them up and partly bridg-



FIG. 24.

× 775.

ing them over. The flattening over of the surface is bringing into view the outlines of the crystalline grains of which the metal is built up.

The fifth slide, fig. 21, shows the effect of further polishing, still in the same direction, on the rouged leather. The emery furrows have now been completely flowed away, and a smooth layer has spread over the whole surface. Owing to their greater hardness the crystalline grains have been left slightly in relief, the intergranular spaces containing a softer entectic have been worn down to a lower level.

The sixth slide, fig. 22, shows the same surface after treatment with solution of potassium

cyanide which has dissolved away the flowed layer and fully disclosed the crystalline structure below.

The seventh slide, fig. 23, shows the surface layer again restored by polishing.

This series of photographs shows very clearly the spreading of a definite layer over the surface by polishing, and its removal by a solvent.

The extreme readiness with which the surface of speculum metal can be flowed in spite of its hard and brittle character at once explains



FIG. 25. $\times 775$.

why this alloy lends itself so admirably to the making of highly polished mirrors.

The next series of slides will show how the same phenomena occur in antimony, one of the most brittle and fragile of the common metals. For these observations a portion from a well-developed crystal of antimony was selected. One of its faces was smoothed by rubbing it to and fro on a very fine file, but the metal was so brittle and fragile that it splintered very badly during the filing. It was then rubbed on the finest emery paper, and while it still splintered, some portions of the surface were ploughed with comparatively smooth furrows. The first slide,

fig. 24, of this series shows the antimony surface as it was left after polishing on emery. The larger of the pits left by the splintering are still visible as rough angular gaps, but the outlines of the smaller pits are closing in and rounding under the surface flow caused by the emery.

The second slide, fig. 25, shows the effect of cross polishing on rouged leather. As in the case of speculum metal, the rouged leather appears to have seized the upper layer of molecules and pulled it over the surface like a skin. The remarkable effect of roofing



FIG. 26. $\times 775$.

or flooring over the ridges and furrows is well seen, while the forms and disposition of the holes in the layer exactly resemble the holes which one would expect to find in spreading a viscous paint or varnish over an irregularly furrowed surface. The smoothing of the surface in the antimony is mainly due to this flooring over effect of the viscous layer, while in the speculum metal the whole of the substance of the ridges appeared to be ultimately flowed down, and the furrows were really obliterated, not disguised.

The third slide, fig. 26, shows that the effect of further polishing is to produce a smooth surface from which the traces of the furrows have almost disappeared. When this smooth surface was treated with a solvent, however, the surface skin was removed and the *fourth slide*, fig. 27, shows that the original furrows and pits are still there.

Returning to the third slide, fig. 26, I would direct your attention to the group of dark pits, some of which it is quite evident have been sealed over by the polished layer. The fifth slide shows some of these pits on a larger scale.



FIG. 27. $\times 775$.

The darker shade of the covering film over the pits shows that it is much less power of reflecting light when it is not backed by the solid metal. The antimony in a film of this thickness is translucent, or perhaps transparent.

The sixth slide, fig. 27, shows a very large pit in process of being covered over by the flowed surface layer. The viscous appearance of the film as it spreads out from the edge of the pit is in harmony with the equally suggestive appearances of the earlier slides.

It may safely be predicted that anyone seeing

these photographs for the first time, and knowing nothing about their origin, would, without doubt, believe that he was looking at a layer which had been painted across a surface while the material was in the condition of a viscous fluid. If this observer were told that the viscous-looking surface layer was in reality a substance so brittle and fragile that it could easily be reduced to a fine powder in an ordinary pestle and mortar, and that it could not be filed with the finest watchmaker's file without splintering, he might be justified in considering that he was being treated to some ingenious paradox, and it would only serve to heighten this sense of paradox if he were further shown that this viscous-looking layer was even harder than the original material, so that it formed an enamel-like protective coating over the more fragile material below.

In my own case the paradox appeared to have only one possible solution, namely, that what *appeared* to have happened really *had* happened, and that we must take it as established that the surface layer had for a brief period been in the condition of a viscous-liquid. Further, as the surface film in its final state differed most markedly in its physical properties from the original crystalline antimony, it must be accepted that the metal in its final form this flowed film is to be regarded as in a perfectly distinct state or condition from the original substance. If these conclusions are correct, then the observations on surface flow have led us to recognise that a crystalline solid can by mechanical disturbance be made to pass through an intermediate mobile phase into an amorphous or non-crystalline state. So important a conclusion could hardly be made of general application till it had been proved experimentally with a great variety of substances; but during the last few years these observations have been repeated in a variety of ways on many different substances, so that I am now

confident the above conclusions can be applied quite generally.

The next set of slides illustrate surface flow in a substance of a totally different character from the metals or alloys. Calcium carbonate occurs in nature as the beautifully crystalline mineral Calc-spar, or Iceland spar. These crystals are familiar to the optical physicist, to whom their doubly refractory property, combined with their transparence and freedom from colour, has made them invaluable. For other reasons Calc spar has been of great value in researches on surface flow. By careful selection it is possible to find crystals which, by cleavage,

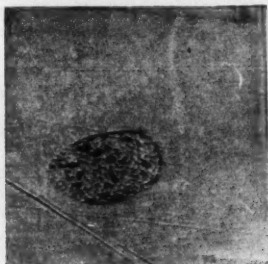


FIG. 28. x 775.

can be made to give surfaces of high natural polish, and as the operation of cleaving can be carried out so as to avoid any rubbing of the cleft surfaces, it is possible in this way to obtain surfaces which are crystalline right to their outside layer of molecules. It is obvious that surfaces of this description are very valuable for purposes of comparison with those which have been artificially polished or otherwise prepared.

When a fresh cleavage face of a calc spar crystal is lightly etched by placing on it a

drop of very dilute hydrochloric acid the surface may, or may not, show a slight depression where the acid has acted, but it shows no regular markings. But if the surface, before etching, has been firmly stroked a few times in one direction, with the point of the finger covered with soft chamois leather, the effect is altogether different when the drop of acid is applied. The drop spreads slightly, flattening to a hemisphere; after 10 or 15 seconds it is removed by touching it with a torn edge of soft filter paper. The pit thus produced has well defined edges, and its flat bottom is covered with furrows and ridges running in the direction of the strokes with the finger.

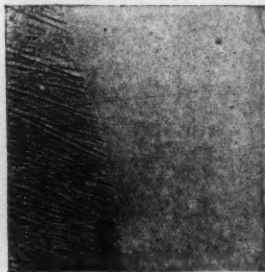


FIG. 29. × 215.

The first slide of this series shows a crystal which has been treated in this way. Two etched pits are shown, the larger of which was about two millimeters in diameter. The magnification is very small, as the object of this photograph is only to show the general appearance of these pits in relation to the crystal face as a whole.

The second slide, fig. 29, which is at a magnification of 215, necessarily takes in only a small part of the etched portion, but so as to

include the unetched surface as well. The unetched portion has a smooth glass-like surface; only a few faint scratches being visible under the most searching examination. Faint cleavage lines crossing the face indicate that the stroking, or polishing, had not been parallel with any of the natural cleavages. The polishing furrows, which are disclosed by the removal of the smooth surface layer, show that the passage of the leather over the surface had disturbed the upper layers to a considerable depth, while the perfect smoothness of the unetched

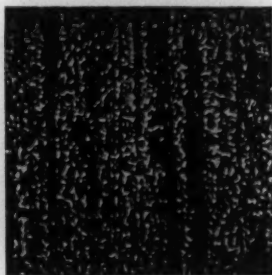


FIG. 30. $\times 700$.

parts shows that the mobile layer which resulted was sufficiently deep to flow over and mask the ploughed furrows. The number and character of the flow lines disclosed by etching with acid is to some extent influenced by the number of strokes given, but these lines were distinctly developed by a single firm stroke in which it was roughly estimated that the pressure exerted did not exceed 4 lbs. per square inch. In this case, while the flow lines were obviously fewer per unit of surface the depth of penetration was not obviously shallower. Some indication of the depth to which the

ploughing has penetrated is given by the additional time required for the acid to dissolve and remove all traces of the disturbance. In the course of a research which is still in progress, careful measurements have been made of the thickness of the flowed layers and the special characters of each successive stratum have been studied. These observations throw further light on the subject of surface flow. When I was asked some months ago to give a paper to the Optical Society, this particular subject was selected in the expectation that it would be pos-

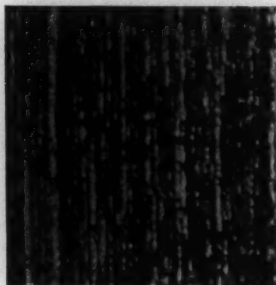


FIG. 31. $\times 700$.

sible to lay the results of these more recent observations before you; but I regret that the pressure of other work has made it impossible to do so.

The third slide shows the effect of longer continued etching, the ploughed lines are now very few, only the deepest having survived. The crystalline lamellae are now distinctly seen cut through by these deeper furrows or scratches.

The three substances which have been selected for the establishment of the theory of surface flow all have characters which place them far

apart from the class of soft ductile substances. The reasons for this selection are obvious, for if the theory can be established in the case of the most unlikely substances its application to the more likely follows as a matter of course. It has been tested by observations on extremely hard substances, on quartz in the form of rock crystal, on agate and even on the diamond, and in every case the results have been entirely confirmatory. Obviously the method of attack has to be modified to suit the particular material, and the phenomena are not all equally suitable for demonstration to an audience.

The ductile metals present characteristics of their own which give them a special interest. Owing to the freedom with which they respond to surface ploughing, the surface tension effect can be observed on a bolder scale than is possible with hard brittle materials.

Fig. 30 is a gold plate on which the slight flow lines left, after polishing with the finest emery, have been etched so lightly as only to remove the outer vitreous layer. The ridges are now partly broken up into rounded, or drop-like forms, while the surface, as a whole, shows a structure similar to that developed on similarly etching a flat polished surface of gold or glass—figs. 12, 13, 16, 17.

Fig. 31 shows the flow lines left on a lead surface which has been cut with a knife. The surface has not been etched, but the drop-like forms of the ridges have been exaggerated by the use of an obliquely incident illuminating beam.

The foregoing illustrations have shown that, alike in the hard non-malleable speculum metal, in the highly brittle and fragile antimony, and in soft brittle calc spar, the production of a polished surface involves the liquid-like flow of a thin layer on the surface. In some respects glass is unlike any of these materials; it is harder, and it is not supposed to be crystalline. The adjective "vitreous" is indeed generally used in contradiction to "crystalline." Under

favourable circumstances the natural surface which results from the cooling of glass articles from the fused, or partially fused, condition, may approach very closely to the state of perfect polish. In this case the molecular mobility due to fusion, or softening by heat, brings the outer skin under the influence of surface tension. The de-vitrification to which some kinds of glass are peculiarly liable during prolonged heating, is partly due to crystallisation of the silicates of which ordinary glass is composed, and partly to decomposition of silicates at the surface by

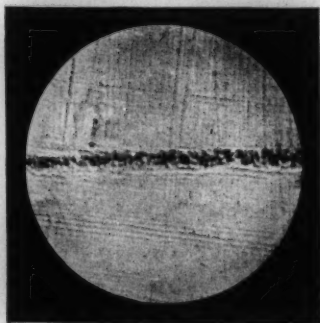


FIG. 32. $\times 1000$.

the hot flame gases which contain oxides of sulphur. The existence of a definite skin on fire-glazed glass can be shown by lightly etching the surface with hydrofluoric acid vapour diluted with air. The surface is only slightly dimmed by this treatment, but the microscope shows that the vitreous reflecting surface has been completely removed, disclosing a finely granulated structure immediately below that surface. The photographs of etched glass have already been shown—figs. 13, 15, 17.

These illustrations show that fire-glazed glass is covered by a thin surface layer which in some respect, at least, is distinctly different from the substratum over which it is laid. All glass cutters must be aware that this surface skin is very decidedly harder than the body of the glass underneath. When the surface of fire-glazed glass is first attacked in emery grinding, it resists the action of the abrasive very strongly, and if too fine powder is used, parts of the glaze are left on the surface practically untouched. The glass once freed from its surface skin responds more rapidly and with greater uniformity to the action of the abrasive.



FIG. 33. × 100.

From what has gone before we have seen that the layers of molecules on the surface of various hard brittle substance can be made to flow like a liquid so that either the smoothest of flow lines or flattest of liquid-like surface may result. The next slide will show that in one of these respects at least, glass is no exception to this rule. This glass surface, fig. 32, was held against a polishing disc armed with the finest

emery paper, with the result that fine flow lines have been developed all over the surface, while at the point where these flow lines cross, a scratch, which had been made previously, there are distinct signs of an attempt to bridge the gap by the flowing glass (compare fig. 19). In this hard substance the layer of molecules, which can be kept in fluid motion by the polishing agent, is, necessarily, much thinner than in softer substances, in speculum metal, for instance, but all the observations point quite clearly to the reality of the flow in this case as in the

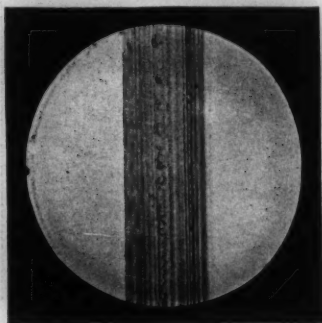


FIG. 34. × 700.

others. When glass is marked by the passage over it of a hard point, it may either be scratched, or furrowed, or cleft. A *scratch* is caused by the splintering of the glass along the track over which the hard body has moved. A *furrow* is ploughed when the hard body is so formed and guided that its points lay hold of a layer only a few molecules in depth. A certain amount of the glass is shaved off, but the perfectly smooth coating of the groove which is left shows that the surface layer has passed through

the mobile or liquid condition. A cleft results when a fine wedge-like point is drawn along the surface. The entry and passage of the thicker part of the wedge may result in furrowing or splintering at the outer surface, but these are not essential features of the true glass-cutters cleaving scratch, which, to be effective, must have forced the glass apart till a cleft is started. The photograph, fig. 33, shows a splintering scratch at a low magnification. The conchoidal pits, which are irregularly broken out along the

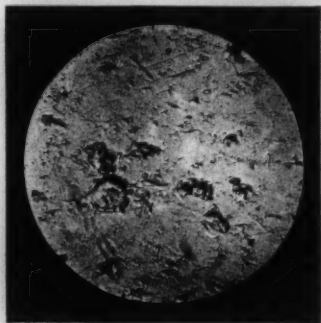


FIG. 35. $\times 700$.

track, are very characteristic and will be referred to again in connection with another photograph. Even in this rough scratch, it is not difficult to detect traces of ploughing and flow. The next slide, fig. 34, shows a series of grooves ploughed by drawing the edge of an uncut diamond along the surface under a slight pressure. The magnification in this case is high, about 700 diameters, and the resolution is also good, if reduced to the magnification of the preceding slide it would only show as a narrow faint trace. The smooth-flowing sur-

face of the grooves is quite comparable in this respect with the flow lines on speculum and other metals. The lines ruled on spectrum gratings ought to be single, clean furrows, free from broken edges or interruptions of any kind. The next slide shows a single furrow, along one side of which the edge has been slightly broken and distorted, but without any true splintering. The next three photographs show a cleaving or cutting scratch. In the first, which is at a low magnification, the point of entry of the



FIG. 36. $\times 700$.

diamond is shown. A certain amount of splintering has occurred as the diamond was pressed through the surface layer, but along the track there has been no free splintering of the edges. The track itself is the straight but slightly broken light line, and the dark band which follows it is the shadow caused by the cleft which has been opened in the substance of the glass below the surface. This explanation of the dark band could not have been arrived at from this photograph alone, but a closer examination with a lens of much higher resolving power reveals

the fact. By focussing for the surface with this lens the light broken band is brought out in greater detail, and it is seen that it is a complete track with practically unbroken sides. The broad band of shadow remains, but it is now seen that it completely lacks definition along its outer edge, which is, therefore, out of focus. By focussing down on the outer edge of the shadow, it comes out quite sharply, and it is found by measurement on the fine adjustment that this boundary is .1 mm. below the surface. The

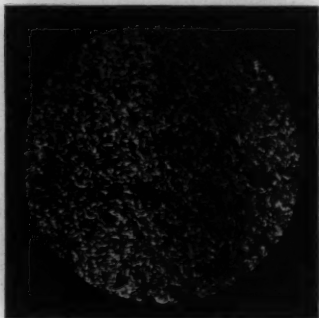


FIG. 37. $\times 700$.

cleft has penetrated the glass at a slight angle, so that there is almost total reflection from its surfaces; the broad dark band is, therefore, the projection of the shadow of the cleft. The furrow made by the diamond appears to have been filled up by flow as the diamond passed along. The width of the furrow is about $1/200$ of a mm. From the study of the various ways in which scratching may occur we are prepared to pass on to consider the various steps involved in the grinding and polishing of glass. I have prepared a set of slides from photographs made at

various stages in these operations. Most of these were made with a 4 mm. objective having an N.A. of .95.

The large part played by splintering in the grinding of glass at once marks it off from the other examples of polishing which have already been illustrated. Among the metals the behaviour of antimony most nearly resembles that of glass, for in its case also, splintering occurs very freely in the earlier stages of grinding; but in antimony this is, to a great extent, counterbalanced by the freedom with which the

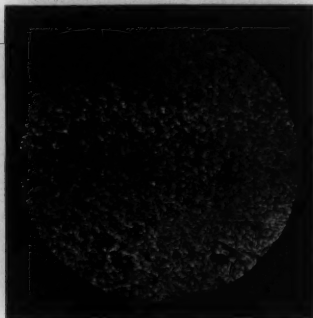


FIG. 38. $\times 700$.

metal flows during polishing, thus filling up, or bridging over, the pits which have resulted from splintering. While splintering plays a very large part in the grinding of glass, ploughing plays only a small one, at any rate, till the later stages are reached. It has been shown that from the nature of the case only a very shallow furrow can be ploughed in so hard and brittle a material as glass, and there is great danger that splintering may occur even in the final stages of grinding. After the actual photographs have

been before you, we shall be better able to discuss these questions.

The first slide, fig. 35, is a photograph at a high magnification of a piece of good commercial ground glass. This is merely shown for comparison with the following specimens, which were all hand ground, with emery powders of known degrees of fineness. The next slide, fig. 36, shows the first effect when moderately fine emery is applied between two glass surfaces which are being ground together. The surface is indented in many places by the points of the

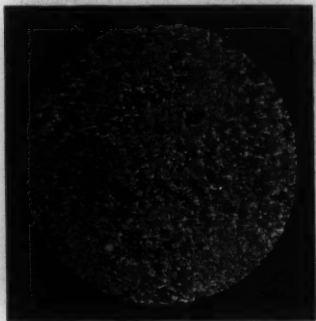


FIG. 39. × 700.

emery grains, and in some cases it has been splintered and pitted. A considerable part of the original skin is, however, still intact. In the next photograph, fig. 37, the whole of the surface skin has disappeared, and the surface is pitted all over, some of the pits being of very large size. In the next slide, fig. 38, smaller pits have for the most part taken the place of the larger ones, though one or two large pits are still visible; definite traces of flowing action may now be detected. In the next slide, pitting

has ceased, and very marked flow has occurred. The outlines of the larger pits are assuming smooth, rounded outlines instead of their former irregular forms. Further results of flow are visible in the next slide, fig. 39; the surface still seems to be very irregular, but there no pits which do not show evidence of partial filling up or covering over. The flowed material is marked by the sinuous surface tension forms which it has assumed. At this stage, as re-

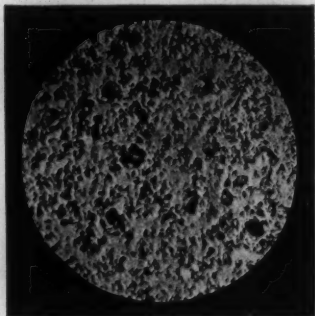


FIG. 40. × 700.

peated grinding with the finest emery did not appear to produce any improvement in the appearance of the surface, emery powder was abandoned and fine wet rouge was applied by means of a hard wood rubber. The next slide, fig. 40, shows the remarkably quick effect of the rouge polishing, the rocky-looking texture of the emery surface has disappeared, and has given place to large patches of a structureless viscous-looking material. The larger pits are now brought out with striking distinctness. In the next slide, fig. 41, these effects are carried to a further stage and the number and size of the

pits is considerably reduced. The rounded edges of the remaining pits show unmistakably that the flowed surface layer is covering over and masking the irregularities below, just as it did in the case of antimony.

The final stage of polishing is reached when all the pits have been covered over. At this stage there is no further structure to photograph.

The emery used in these experiments was prepared by shaking up 15 grams of levigated emery flour with half a litre of water. After

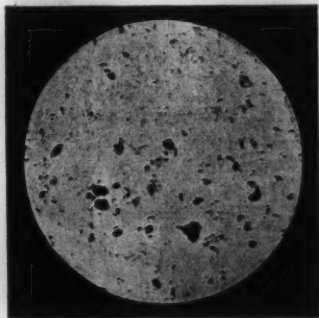


FIG. 41. $\times 700$.

settling for four minutes in a cylindrical vessel the top half of the liquid was run off and settled by itself—the product was set aside as “four minute emery.” The residue in the vessel was filled up with water to the original volume, shaken up and settled for seven minutes, when the top half was decanted and settled, the product being set aside as “seven minute emery.” This set of operations was twice repeated, and the products were set aside as “12 minute and

20 minute emery." A thin film of the 12 minute emery smeared on glass and dried, showed that it contained crystalline transparent masses up to .04 mm. in diameter. After this emery had been ground between two pieces of glass till its cutting action became very feeble, it was smeared on a glass slip and allowed to dry, the larger angular masses had completely disappeared, giving place to much smaller rounded, gravel-like particles, the larger of which measured about .001 mm. Mixed with these was a considerable quantity of glass which, so far as its structure could be resolved by the microscope, appeared to be in fine film-like particles.

Attempts have been made to study the structure of fine rouge. It was found that the rouge left in suspension in water after 10 to 15 minutes, chiefly consists of particles small enough to possess pedetic movement. Some of these particles examined by an objective of the highest resolving power appear as transparent red particles of rather irregular shape. Owing to the extreme activity of their movements in water it is difficult to estimate their dimensions with accuracy, but their average diameter may be taken as from 500 to 800 micro-millimeters. The pedetic movements are not confined to single granules of this size, but groups containing a number of these particles possess activity though of a lower order than that of the single units. The result of numerous observations, the detailed description of which would be out of place on the present occasion, go to show that these visible microscopic particles are really aggregates of very much smaller invisible units, and that the special qualities of rouge, as a polishing substance, largely depend on the hardness of the minuteness of these ultimate particles. The *visible* aggregates are more or less casual associations of these finer units under the cohesive forces; they are not, therefore, really hard in their collected form, if they were

they would act like emery or other rough powders, and produce furrows and scratches. The researches which we are discussing this evening show that the flow which is necessarily involved in all true polishing is a molecular operation, the layers of the substance which are involved are in some cases known to be only a few molecules in thickness. It is due to the almost molecular fineness of rouge that the necessary molecular contact over comparatively large areas simultaneously can occur. The seizure of the surface layer of metal or glass in rouge polishing is something altogether different from the ploughing action of the coarser abrasives.

In the ploughing by emery, or by the point or edge of a steel tool, molecular movement and mobility often of a considerable mass of the substance always occurs in the immediate neighbourhood of the moving point, but the essential feature in these cases is that the disturbance is more or less convulsive, the material being turned up from considerable depths judged by the molecular standard. As the moving point passes on, the disturbed material subsides as a thick liquid would do into ridges and furrows of smooth outlines, at least in detail.

The complete obliteration of the furrows by the liquid surface layer only occurs when a sufficiently large number of points on the polishing agent and the surface are simultaneously in contact, in other words, if something like a continuous sheet of molecules of the polishing and the polished surfaces are in contact. In polishing the furrowed surface of antimony across rouged leather by hand there is a perfectly definite feeling of "grip" as the surface of the metal is seized by the rouged surface. This feeling is quite distinct from that produced when the rouged leather is replaced by the finest emery paper, even though in the latter case more actual force is expended in pushing the metal across the rougher surface.

In the polishing of glass, figs. 36-41 show very

plainly the distinction between the surface flow caused by emery and that caused by rouge. While the finest emery particles undoubtedly cause flow as they plough through the surface layer, yet the depth to which they penetrate disturbs the under surface and impairs the smoothness of the final surface layer. The rouge particles, it may be supposed, hardly penetrate below the surface, but coming into almost molecular contact with the sheet of molecules on the surface, drag it off like a skin. The fresh molecular layer left by the removal of the skin retains its mobility for an instant, and before solidification is smoothed over by the action of surface tension, thus producing the liquid-like surface which is the necessary condition of a perfect polish.

HIGH POWER CYLINDRICAL LENSES.

BY MR. H. L. TAYLOR.

Read on December 19th, 1907.

THE question raised by Mr. Chalmers* may be looked at from a different standpoint with, perhaps, certain advantages. By taking in turn each meridian of the eye which corresponds to the two principal meridians of a sph. cyl. lens (those of highest and lowest refraction), we may consider the refraction as merely spherical for that meridian.

The far-point of an eye, emmetropic, hyperopic, or myopic, as the case may be, and the retina of that eye are conjugate foci. In **emmetropia** the one focus is at ∞ , in myopia it is between the eye and infinity, and in hyperopia it is beyond ∞ (i.e., it is at the back of the eye), and the greater the hyperopia the nearer is it to the eye. Any lens which, in a certain position before the eye, corrects the defect for distant vision, will have its principal focal point coinciding with the far-point of the eye, so that the distance from the lens to that far-point is the focal length of the lens. Now, assuming that the object viewed and the lens are movable we can bring them into such a position that the lens shall be midway between object and image (between the conjugate foci), and in this position it will be the weakest lens possible for the fixed positions of the conjugate foci, the distance from each to the lens being twice the focal length.

Any movement of the lens will necessitate an increase in its power, to still retain the fixed positions of the conjugate foci, and this accounts, on the one hand, for the increase (shown

* Chalmers, S. D., Transactions, 1906-7.

in the table) for the equivalent lens at the cornea, for, clearly, so long as the distance from lens to object is more than twice its focal length, moving the lens increases its power, but, on the other hand, when this distance is less than twice its focal length, it diminishes the power. An inspection of the table shows that with the object at 250 mms., the change comes with + lenses, between 7 D. and 8 D., in accordance with the above, but in - lenses the distance between object and lens is always, *algebraically*, less than twice the focal length, so that moving a - lens results in less power for all distances of the object.

In actual practice the added cylindricity shown in Mr. Chalmers' calculations is generally partially, and frequently completely, neutralised by the position of the lenses, or else by some adaptation by the wearer. Where a correction is worn constantly, and objects at a reading or working distance are viewed by eyes in or about the primary position, there is only the effect of convergence of the visual axes to consider. In the particular case of 250 mms. distance the visual lines are angled to the normal to the lenses merely about 6° or 7° , with ordinary interpupillary distances.

When, however, the eyes are depressed for near work, in addition to the convergence there will be a very considerable angle formed between the visual axis of the eye and the normal to the lens, from 10° to even 30° , according to the stature, bearing, or habit, of the wearer. When a special "reading" correction is used the lenses are usually angled before the eyes, but the usual 10° , or so, of angling, is quite insufficient in many cases, and the visual axes are still angled to the normal 10° to 20° .

Bull, Green, and others, have tabulated the equivalence (in the same position) of angling sph. and cyl. lenses in front of the eye. We may, with small error, consider that for every 1 D. of a spherical lens angled 20° a lens of 1.04

D. sph. $\ominus$.14 D. cyl., results. With a cylinder angling in the direction of the axis has no effect, but angling across the axis gives greater changes than the spherical; for every 1 D. of the cyl. an angling of 20° gives 1.18 D., while 10° gives 1.04 D. In fact, the change in the cyl. power with such angling is approximately as great as the change in the *sph. power only* of a sph. lens angled twice as much.

Angling a lens increases its power so that the added cylindricity is always such that its axis is at right angles to the meridian of the tilting, that is, it coincides with the axis of rotation.

Considering cases where the correction of a defect is "with the rule" it is clear that with + lenses the whole or part of the cylindricity of the tilting forms with part, or the whole, of the added cylindricity, due to nearness of the object, a spherical lens with some cylindricity remaining. Thus, in the example of a + 4 D. sph. $\ominus$ + 3 D. cyl. at 15 mms. from the eye, we should find that a tilt of 15° would leave the cylindrical power practically unaffected, while + .5 D. is added to the sphericity, and this possibly explains why astigmats hyperopic with the rule, do not, if the distance correction is worn, appear to be presbyopic so soon as they should. With the same defect, "against the rule," the cylindrical element is increased very much by the tilting, and only lessened to a very small extent by the effect of convergence, a result probably explaining the trouble often experienced by wearers of lenses for this defect.

Astigmatic myopes "with the rule" will benefit by the tilting of the visual axis, because the added cylindricity will counteract the lessening power caused by nearness of the object, but where it is against the rule the result will be a considerable variation from what is required.

I do not quite see how the effects of distance influence astigmatic accommodation, because this function, or a simulation, caused in whatever way it may be, is supposed to be exercised

by the uncorrected eye in distant as well as in near vision. A simple + sph., for distance, given instead of a + sph. cyl., would cause an increase of the function for near work by the obliquity of the visual axis to the lens, provided the defect is with the rule, but would neutralise it if against the rule, but, presuming that the lenses supplied are correct for distant vision, some adaptation or compensatory accommodation is absolutely necessary for them to give the same visual acuity for near work in the same plane of vision—a condition which is not frequently found, and so far as I am aware there are no facts to support the idea of an adaptation of the crystalline lens for this particular condition.

There are other factors which may influence the effect of the change in power of lenses when near objects are viewed, notably in reading, because vertical lines, which are so predominant in distant objects, become of less value in reading printed type. The subject is a large one, because the effects vary with the nature of the defect, the position of the faulty meridians, the form of the lenses used, and the habits of the wearer.

"THE RELATIVE VALUE OF PARTS IN SPECTACLE FRAME MANUFACTURE."

BY MR. H. L. TAYLOR.

I do not propose to deal with the joints, eye-wires, screws, etc., which compose a spectacle frame, but rather with the principles underlying the construction of a suitable frame for given requirements. So that we may take it that I shall deal with the parts of a whole rather than with the separate entities.

Now, there are two persons to consider in this matter. The one is the frame fitter, and the other is the workman. In many cases the first, from lack of technical knowledge, is unacquainted with these principles, and quite unable to convey the idea of the exact requirements. On the other hand, the workman, with little knowledge of facial formation, and with skill born of experience alone, is very liable to make somewhat similar errors to those of the frame fitter. We have, then, what I may call two links of a chain. The principles which should be understood by both are the missing links of that chain, and these same principles are bound up in the relative value of parts in spectacle frame formation.

We will consider first what I will call the three primary planes for a symmetrical face. These three primary planes correspond to the three dimensions in space. I can perhaps show you that better by an illustration here, and by the blackboard. You will notice that I mentioned a symmetrical face particularly, and to start with we must suppose that the eyes are in the primary position—that is to say, that the visual axes will be parallel and looking towards

the horizon. Now, there are three planes to consider with regard to the present purpose. Here is a model of an eye, and it is held by an arrangement which allows of certain rotations. Supposing we have a similar eye to this in the position which it should occupy here. A spectacle is placed in front of those eyes, generally speaking, at some 15 mm. from the cornea. This position is known as the anterior focus of the eye, and a spectacle lens occupies what we might call the second-best position when it is placed there. The first position—the best position—would be in contact with the cornea. Now one of these planes, the anterior focal plane, is in front of the eye through those two anterior focal points. So that really, if I hold the eye in this position, and imagine a similar one here, the anterior focal plane would run in this position, just as if it were a straight piece, we will say, of cardboard in front of the two eyes. The second is the horizontal plane through the polar axes. I have used the expression “polar axes” in preference to the old one, “optic axes.” The polar axes of the eye are the two lines—one for each eye, of course—running from the central point of the cornea to the posterior pole of the eye, theoretically. They are really the axes about which the refracting media of the eye are centred. Now, you can quite see that we may have a plane running through both of these. That is the horizontal plane. The third one is the medium plane of the head. The medium plane of the head does not touch either eye. With the symmetrical face (and you will notice that I specially speak of the symmetrical face) the medium plane of the head passes exactly through the centre of the nose; so that it is just as if you cut the head in two downwards. If you will notice, there is a point of intersection for these three planes, and I might illustrate them on the blackboard in this way. Suppose we are looking down at the two eyes, having

one here and the other there, the medium plane of the head would come exactly between, and would bisect the nose as it would appear there. The anterior focal plane would come about here, and the horizontal plane would be exactly in the plane of the blackboard. So that these planes intersect at that point. Now, all these planes are useful so far as the measurements of a spectacle frame are concerned. The anterior focal plane is that in which the lenses lie when they correct ametropia. In the horizontal plane you will find the side-to-side movements of the eyes take place during convergence without depression or elevation. That means that if we suppose this eye to have a motion only in these two positions, the eye will be moving in the horizontal plane. Of course, when we had a depression or elevation, the plane would depart from the particular position which we have chosen—or we may regard it as the polar axes of the eyes departing from the position of the plane. This plane is the original plane for the perception of relief. We get our ideas of relief in binocular vision primarily from movements of the eyes in this plane. You will notice with regard to our spectacle frame that the centre lines of the joints and the sides lie in this horizontal plane. This, in a great measure, is true even if the plane is angled. So that an angled front will really mean a motion of the anterior focal plane in relation to the horizontal plane.

Now let us consider the medium plane of the head. All points necessitating equal accommodation and convergence on the part of the two eyes lie in this plane, and in this one only. Consequently the far-point and the near-point of vision are both in it. You will see that the point—whatever point may be chosen—so long as it is in this plane necessitates equal accommodation and equal convergence on the part of the two eyes. Now we may regard these planes as concerning distant, and also near, vision, and you will see that the medium plane never

moves. It is a fixed plane with regard to the head. But the other two planes may move. The horizontal plane moves with depression of the head, and with depression of the eyes it moves still more. So we may regard it in a primary sense and a secondary sense—a primary sense for the primary position, and a secondary sense for the near work and reading.

Let us now take, for a few moments, a spectacle frame. In this there are two main divisions of parts. The one is the front, and the other division will be the sides. The front of a spectacle is intended to hold the lenses in the correct positions, and at the necessary distance from the eyes. The sides are for the purpose of holding the spectacle on, and for holding the spectacle on only. This relation is so important that a departure from it will result in bad work either on the part of the frame-maker or of the frame-fitter. As the frame must be held up, we will consider first the sides, and with these you may have two extremes—you may have a short side which is stiff, or you may have a long side which is weak. Now you may also have a stiff front with a short stiff side which will hold on the face, and no doubt many of you will remember the old English type, which is practically extinct now, that depended entirely upon this short stiff side; but it also had a short stiff front. This spectacle, which was sold very extensively at one time, had a peculiarly rigid front, and that accounted for the fact that it practically never fell from the face—the stiff front combined with those short stiff sides. On the other hand you must have noticed that most of the American frames had long sides. There is a reason for that, quite apart from the fact that the Americans may like long sides. Their sides are made out of stamped metal; they are comparatively weak; they must be long. I do not say that their fronts are always weak, but the fact that the side is long depends upon the fact that it is also weak. You will see that the

long side really becomes a curled side when it is carried beyond the ordinary length of a straight side, and doubtless many of you will remember the bent side, as it was called, which was made longer than the ordinary straight side, but shorter than the curl. No matter what type you have, it is necessary that that side upon the face, from the joint to the ear, should be perfectly straight, that is to say, it should lie in the horizontal plane. This is a part which concerns the fitter as much as the frame-maker, because at present there are very few curl and cable sides which are properly fitted. The frame-maker puts a conventional bend at the end of his side, which would give you the impression that it was to fit on a pill-box, but, as we all know, no human ear ever has that shape. It is quite unnecessary that this shape should be made to fit upon an organ which is of a totally different shape, and it is simply made so for the sake of appearances. If you come to examine the back of the human ear, you will find that it is usually different in shape from the curl which the frame-maker gives to the side that he has in hand. Supposing that you get this shape at the back of the ear, then all that is necessary is a perfectly straight line there, no matter what the strength of your side, or whether it is curl or cable. And the fact that this part bends is rather important wherever you see it, because it is very certain that the bridge is incorrectly fitted. It is one of the most certain signs of an incorrectly fitted bridge—the fact that you get a bend in the side. This bend in the side means a tension—a tension on some part of the frame—and if you consider where that tension must find relief you will see that it can only be in the front, and as the front is rigid it must of necessity follow that the bridge is at fault. A stiff front means that the lenses are in the anterior focal plane of the eye. Suppose that you have a frame which has stiff sides and a weak front—when it is put on the

face you find a bowing of that front. You see it here in a cheap German frame that I have. You will notice that these sides are peculiarly harsh and stiff, and that as soon as I bend them apart to put them on the face, the front bends outwards in just the opposite position from what it should, if it should bend at all. Another point is that in a frame like this, with a peculiarly harsh side, in all probability when it is put on the face it rides up. If you put this frame on you cannot, in most circumstances, keep it on the nose—it will jerk up, and the stiff sides are the cause of that.

Let us consider for a few minutes, now, these three planes. We take measurements from them—sometimes unconsciously. The medium plane becomes especially important in cases of asymmetry, and it is a remarkable thing that if you measure a number of faces, where you have intelligent people to deal with, you will find that 75 per cent. of them are asymmetrical. I have taken a great number of these measurements with an instrument for which we are indebted to Mr. Dixey. I have in conjunction with this had a clip which grips the nose, and grips a portion of the nose which is usually fairly central, and I find that in over 75 per cent. of the cases the right eye is further removed from the medium line of the face than the left. So that this medium plane becomes very important in asymmetry, and as asymmetry may take two or three different forms you can quite see that we must have some such point, or some such plane, from which to measure in one direction towards the right eye and in the other towards the left.

With regard to the horizontal plane you can see that from it we measure the height of the bridge, and there are other details which we take from that as a starting point. The anterior focal plane is useful in some other ways. We do not measure the projection of a bridge quite from that, but we take it almost as a starting point, because we measure the projection of

a bridge—or we should measure it—from the anterior plane of the lenses. Still, for all practical purposes we may take this anterior focal plane as of great value in such measurements as those. In the spectacle front the bridge is the steadying factor, and in this respect the W bridge is the best; the C, or crank, bridge is of less value; the X and K being of still less value. If the bridge does not steady the front upon the face, the sides have to take up part of its duties, and so the joints rock, and the sides are bound to get awry. The straight side with the C bridge is the greatest offender, simply because of this fact, and it is worse with the C bridge correctly fitted than it would be with the W bridge.

I will deal with the W and C bridges separately, because they are constructed differently. To commence with, we will take the nomenclature. We must have something by which to specify these various parts of a bridge. We have first the crest and the slope. The crest of a bridge is obvious. The slope is the slope of this crest, naturally. And I want you particularly to notice that this slope is measured in degrees from the anterior focal plane. That is to say, if we have the plane here (vertical), we measure that slope as a slope of so many degrees from it. Suppose I represent the W bridge in profile so, and I have here the anterior focal plane, which is the plane of the lenses. We measure the slope from that plane, so that whatever the slope is to this line, practically to the front of the spectacle, that is the slope of the crest. If the crest could be made in that position (horizontal) it would be 90 degrees. When it is made in the slope of the frame itself, it is nothing. Such a bridge is possible—in fact, such a bridge is frequently made, when the shanks are about here, and the bridge itself lies in the same plane, and then they are either in the horizontal plane or in a plane parallel to it. These portions (from the crest downwards)

are best known as the sides of a bridge. This is somewhat confusing, because the word is applied to another part of the frame, but I am afraid there is no better term. Now we have another slope to consider in the slope of a W bridge, and this is generally known as the rake of a bridge. You see it here. If you look end on at the bridge, it has a slope which is known as the rake. There is one very important relation which it is quite necessary to notice here. The slope of the crest and the rake of the bridge are always at right angles in a W bridge, unless for some reason the rule is departed from, which is very rare, and if you notice the way in which the bridge is made you will see that this must be so. This piece of brass was hammered out flat, and then turned up. In the turning up, it is of necessity turned up at right angles to the slope of the crest. That is the rake of the bridge. The base of the bridge you are all familiar with—it is the measurement between these two lower points. The depth is the distance from the crest here to the base.

Now, we have two other parts in a bridge. The one is frequently known as the shank. It is the piece which extends from the base to where the bridge is joined on to the eye-wire, and in the better form of W we have the piece which you see here—a piece which is usually hidden, because it is joined on to the eye-wire. This is variously known as the splice, or the strap. It is a part which the Germans are particularly fond of imitating. If you take a German gold frame, you will generally find that the splice is false in some respect. Either it is made of two separate pieces soldered on with gold solder, or else it is very short, and the gold solder is run round the eye to make it look like the real article. But it is practically never the same as the English splice. I want you particularly to notice that this relation of slope of crest, and rake of bridge, does not apply in the C or crank bridge, unless the crank bridge hap-

pens to be improperly made. Supposing we have the eye-wire vertical, and the crank bridge with 30° slope, as you know if you put it out 3 mm., the rake would meet the eye-wire too soon, and the crank bridge, to fit a nose of 30° slope, we will say, has to be bent round in a slight curve. This is what the workman calls "knocking the crest over." So that you see that in this instance the slope of the crest is not at right angles to the rake of the bridge. For the best result in a W bridge, the shanks of the bridge should lie in the horizontal plane, or in a plane parallel to it—really for cosmetic effect, because if you will look at this bridge when it is soldered on to the eye-wires these parts here will appear end on; you will see least of them—and generally speaking it is considered best to have them in that position. The splice also is of importance, according to where the shanks meet the eye-wire. In some cases it is taken downwards, and in others upwards. The rule, generally speaking, is to splice in the direction which requires least bending, because with the W bridge properly made there is considerable trouble in putting in these various bends if the thing is to be done correctly.

Let us consider for a moment the effect of angling on the slope of a bridge. You can quite see that if you have a spectacle side in the horizontal plane, and a spectacle front in the anterior focal plane, those two are at right angles, and your bridge may be angled a certain amount there, and you will specify it as having a certain slope to the anterior focal plane. We will suppose that you specify the slope of crest as 45 degrees to the anterior focal plane. This slope is generally expressed as angled so many degrees to the front. Now in calculating what angling you must give here, you must take into account the fact that your side is angled so much, really the front being angled that extent.

Having considered bridges, let us consider now to some extent the noses they are to fit.

You will notice in the human nose that the curves predominate, for practically every part of the nose is a portion of a curve, except the slope, and part of the sides. An inspection of the profile is necessary to take the position in which a bridge should fit, and generally speaking, whatever the slope of the nose, you will find a certain concave curve to the brow which may be used to determine the position where you are going to fit your bridge. As a rule it is the lower part of this curve which you will take upon which to fit the bridge. If you get a nose with a very large slope here—a *retroussé* nose—it will come nearer the centre; if you get a high bridge of Grecian type you will find that it is right at the extreme end of the slope.

Now, we have in these noses a curve at the top to form the bridge, and, generally speaking, a slope here, which is straight. As you will see, in this particular bridge that portion is rather considerable. As this curve at the top enlarges in size, generally speaking the straight portion here lessens, until sometimes you will get a bridge which is a portion of a circle—a pure curve—especially in children. This really then becomes the arc of a circle. The correct procedure now is to see how these parts vary in the relative values of the parts, and for the sake of example let us take a portion of the map of England. I will draw here quite a rough outline of the western part. The eastern coast is not so necessary. Now it is a very remarkable thing that you have only to follow the dialects in this portion of the country, and you can from that draw a very fair conclusion as to what will be the average type of bridge. Let us follow some of these dialects. You get one very marked here (North Wales), you get another there, in fact, a portion of this extends over here, and then you get another portion down here (Cornwall). If you notice these particular instances, except probably here, the people are Celts, and they were forced into that posi-

tion by hordes of people of another race which came from the east. The pressure is always from the east, and you can define the characteristics of the bridges in those districts provided that you remember certain facts. Now, these we may call primary influences. There are secondary ones, and some of them are very curious. You get a distinct secondary influence here, we will say, taking Manchester as a centre, and some 20 miles round it. When you come to Yorkshire you get another influence here, and there are some very remarkable cases of secondary influences within other areas. If you take a little portion here, and another little portion here, and one or two others—notably in Norfolk and in Essex—you get quite a different type of nose, and for a very good reason. This particular portion (Pembrokeshire) is known to this day as "the little England beyond Wales." It is really a part with a people who are the descendants of Flemings, whom Henry I. brought over. Here, again, in this peninsula, known as Gower, you get a similar state of things: The people speak English, and they have quite a different type of nose from the Celts. Now, here you get a peculiar combination as the result of mill life. You get the receding bridge—it is a retrograde type. On the other hand, in Yorkshire and in this portion of the country—North Devon, the land of Kingsley—you only have to read his works and you can quite see what noses the people have. They have a peculiar breadth—much the same characteristic as you find here. The Celtic nose was high, and generally narrow at its crest. You find it particularly over North Wales. In South Wales it is not so high; there is a strong admixture of outside elements. In Cornwall you get Celts again, with somewhat similar types of nose to what you have among the North Welsh, but there are some differences. There is generally a difference in the base, for the Cornish nose rarely has a wide base. When you come

to this part of the country (Yorkshire), practically speaking, you always get a wide base. Of course you will get exceptional cases in all these instances, but there is the predominant feature, and it is very clearly marked. In the particular instances of the Flemings or their descendants, you get a peculiar square face, with a nose which is somewhat low. Now, how are we to manage with a number of features like this? It is very certain that we ought to have some system which will express all these variations, and which does not depend on any particular specification given by arbitrary measurements. Let us see how we can express these relations. You have a curve which varies very much, you have a base which varies very much, and you have a depth which also varies. It is expressed as follows: Supposing you represent your base by a line, and your depth by a line, and you have a curve at the top, what we might call an arc of a circle. Now, by joining the base to the circle by tangents, you form a definite figure, and by that figure you can express, practically speaking, the shape of any bridge. Not only so, but you can express asymmetry, and you can express it in several ways. You will notice that we have the medium line of the face, and that you can express departure from that medium line of the face, which should, of course, come right through the bridge. You can express departure either with regard to the extremities of the base here, or with regard to the distance of the eye from that line. I want you now to notice, in addition to this, a rather peculiar feature which perhaps has not been given sufficient attention. It is what is known as the facial angle. This facial angle is by no means a new idea. The old Greeks knew of it very well, and all their best figures were modelled upon a certain facial angle, as they called it. As a matter of fact, if you take a slope, or a line, from the forehead here down to the nostril, and then bring it across to the ear you have formed a

facial angle. The old Greek idea was that the right angle was the typical angle—the perfect angle for the face. And there is some reason for their decision. As you know, we have only to imagine a very small angle, and you can quite see the retrograde face that you get. Of course there the angle almost coincides with the slope of the nose, but if you notice many of the Greek statues, you will be astonished at the striking predominance of the angle. You can see what it gives by the high forehead and the sweep that comes from it. This right angle was the typical facial angle, and we get some rather curious results from this. It is very rarely that you get a case requiring a spectacle bridge where the slope of your crest is nothing—that is to say, it is in the anterior focal plane, and placed high on the eye-wires. So that the person with a perfect facial angle is rarely ametropic. It is a very remarkable thing. I have seen many such people, but I have never seen one wearing spectacles. As you can see, in many of these cases the bridge would probably be 8 mm. or 9 mm. above the centre of the front itself. The rake of the bridge would be parallel to the horizontal plane, which means, of course, that the slope of the bridge would be vertical. With regard to this slope of the bridge, I have three very rough models here, but they are quite sufficient for the purpose of showing what I mean by the slope of the crest of a bridge. Here is a very extreme type. You will see that the slope of crest of a bridge which fits that would be very nearly 80 degrees. On the other hand, if you take the opposite extreme, you have this large curve, which will give you a slope of very few degrees. We may regard another one as somewhere about the normal, with a slope of 45 degrees. Now, with regard to this facial angle, it is sometimes a rather imperfect guide. You must be very cautious of accepting the reading of a facial angle as always meaning the same thing. I pointed out to you a variation

due to mill life in Lancashire. It is a peculiar thing that you will get this perfect facial angle in Lancashire with a retrograde development of nose. But there is always this difference, that you almost invariably get a falling away at one point, and you get this *retroussé* nose, a snub nose, as it were, and although you get your facial angle formed in just the same way, you will notice that the contour of the nose comes inward somewhat, and that the bridge, instead of having projection, as in the true typical facial right angle, will recede here, and will also be lower. This is one of the characteristic features of Lancashire mill life, that the nose tends to recede, although the forehead keeps its position.

I would like to say a few words with regard to bridges, and the making of bridges by hand and by machine. You can quite see that it is practically impossible to make bridges to fit these many variations by machine. It is absolutely impossible, for the simple reason that you would want such a huge number of models. And even supposing you made them, you would not get the same effect as you do in the case of the hand-made bridge. There are many opticians who resort to pliering. This is, generally speaking, a mistake, for once a bridge is pliered, you lose a considerable part of its stability, so that really every bridge should be made to a particular measurement, and should not be altered from that, if the best results are to be obtained. Taking the slope of the crest and the rake of the bridge in conjunction with the point which I raised earlier concerning the marking of a nose due to the bridge, let us take this (illustrating) as a frame seen in profile, with the rake of the bridge in that direction and the slope of the bridge at right angles in that direction. Naturally, then, the nose will come down here. Now, we will suppose that we have the side along here—this will be in the horizontal plane—and that it is a curl side, coming round there,

we will say. Further, suppose that we have, instead of the perfectly straight line, a bend in the side in that direction. That bend means that there is a tension somewhere. You can quite see that the tendency to straighten—and it must have a tendency to straighten—will cause this lower portion of the bridge to cut into the nose there. The tendency is that way. And this, no doubt, accounts for quite a number of people whom you will see wearing the W bridge, and having the lower part of the nose quite red and the upper part pallid. The reason is that there is interference with the fine capillary circulation. Consequently, the blood rushing along, is practically stopped there, owing to pressure, and part is pallid. I have seen those cases remedied in a few days by an alteration in the slope of the bridge by the fitting of the side. So that very frequently, when you find trouble here in the fitting of a bridge, you must look to this for the cause of it, although it is not always the case that the bridge is in fault. How are we to specify contour in cases of asymmetry? Supposing I take this as a model of the ordinary W bridge. How are we to specify these three relations, the base, the depth, and the apical arc, as we call it, in cases of asymmetry? Asymmetry may be of two types. It may be that the nose is asymmetrical—that is to say, the one side is different from the other—or it may be that the one eye is further removed from the medium plane than the other. Now you can see that if I shift this perpendicular, I take it out of the medium plane, and supposing I take here a base of a certain length, and I decide to put up the perpendicular at so many millimeters from the centre, I shall get a line there which represents the depth of the bridge. Now providing there is sufficient space here, I can put the circle at the apex, and I can draw my tangents in exactly the same way and get asymmetry—*asymmetry of sides of a bridge*. On

the other hand, we get a case where the two eyes are unequally removed from the medium plane of the head, and the bridge may be symmetrical, as this is. In that case I may have, and it will usually be the right hand, I may have that shank extending out further than this one. It really amounts to this, that in such a case this bridge would have been made with one shank projecting outwards in that way. That is asymmetry of position of the eyes, and the matter is important because of the difficulty of expressing it in measurement. If you take notice of the medium plane of the head, you can generally express this without any trouble, because if the base is symmetrical that needs no attention. All that you have to do is to specify the distance of the eye from the centre line which represents the medium plane, and in this case the two distances will be unequal. The bridge is the factor which gives stability to the front. The depth of the bridge is the particular feature of a bridge which gives stability to the bridge itself. As you can see, if the bridge fits upon a certain nose, the deeper you can get that bridge on the nose with comfort, the greater will be the stability of the frame. Another point which is worth noting is that a C bridge and a W bridge fitting the same nose will frequently have a different base, because the W bridge, as I pointed out, has a rake at 90 degrees to the slope of its crest, but the C bridge, which fits the same nose, and must have the same slope, will frequently come round in this position, and so have a measurement from point to point of its base in a different position from what the W would have, and that accounts for the C bridge frequently being less stable than the W in wear.

There is one problem which I will give you before concluding, showing what information we may gain by reason of these planes and the various points in bridge and frame measurement. Supposing we have in question a rimless

spectacle, we are given only the height of the bridge, specified as 10 mm., the projection being specified as nothing. We are told that the slope is 20 degrees. Now that is all we require to know for this particular case. We do not want to know anything about the contour of the bridge, but we do want to know what shape of bridge we are going to use for that frame. You will see that with a rimless spectacle, practically speaking, we must drill the holes all in the horizontal plane. That is to say, we have one hole there in this lens, we have another there, and so in the other. Now, then, we have a height of 10 mm., and no projection, with a slope of 20 degrees. It comes to this—that the slope would be something like that, and we must bring our bridge down to a point somewhere here to touch it, because of the hole in the lens. If you bring the rake of your bridge along here, you can see that wherever you bring this to, your shank will be very awkwardly placed. You cannot get it down to the drill-hole; you will have to bring it down there if you adopt a W, which makes an exceedingly awkward bridge. Now, supposing we see what the C bridge would be, assuming these measurements come out approximately the same. It is true enough you can make a C bridge, because you have that slope, but in a rimless spectacle the C bridge would be ridiculous—you would get no stability. So that we must devise some particular form of bridge to suit this particular case, and the best result would be a combination of the two. Supposing we put here a slope in that direction, and, having the hole to which the bridge must eventually be attached—the drill hole—here, we can bring the bridge sides round in a curve, and then we can take our shank straight away there. You see that bridge is really the combination of C and W. But you would have to note, in giving measurements of the bridge, that this position no longer holds. The measurement of depth is taken from here

to there; but with such a nose as this would fit you would find that the slope of the sides along here would be practically identical with the slope along there, because that nose must very nearly approach the ideal nose, having the angle at 90 degrees.

NOTES ON RESOLVING POWER AND DEFINITION IN OPTICAL INSTRUMENTS.

By R. T. GLAZEBROOK, M.A., F.R.S.

THE undulatory theory of light gives a complete account, if we can utilise it, of the action of optical instruments in forming images. Lord Rayleigh has applied this to the telescope, the spectroscope and microscope, and it is my intention to follow his work, referring, however, in a number of points to Abbe's remarkable series of papers.

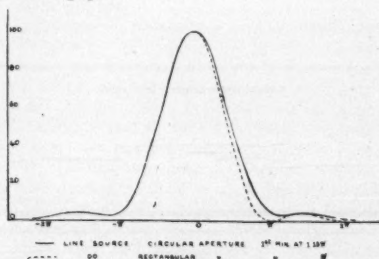


FIG. 1.

Consider the image of a distant line of light formed by a lens with a rectangular aperture. The image formed in the focal plane has maximum illumination at the point 0, fig. 1, where the axis of the lens cuts the plane; it falls off rapidly on either side, reaching a minimum, zero, at the point marked π , and passing through a series of maxima and minima to zero. This distance from 0

to π subtends at the lens an angle equal to that subtended by the wave-length of light at the distance of the breadth of the aperture, for light from half of the aperture reaches this minimum point half a period behind that from the other. The image of the line is not a narrow line but a band of gradually decreasing intensity, and is narrower the wider the aperture. The possibility of seeing the line as a blur when the aperture is narrow is in large measure a question of the brightness of the source and of the total light which the aperture lets through.

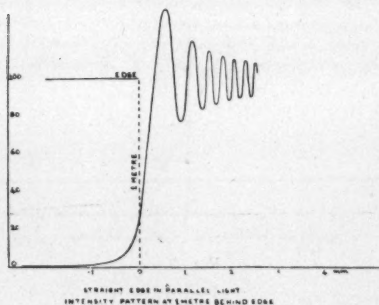


FIG. 2.

For narrow apertures a series of diffraction lines is formed.

Again, take a straight edge bisecting the field, one half being dark, the other bright; consider first a plane wave passing by this falling on a screen at a considerable distance behind the edge; we have to one side of the position of the geometrical image of the edge light gradually fading off, to the other a diffraction pattern as before with maxima and minima of intensity. In both cases appearances in the field depend on the boundary of the aperture.

Now take the case of a lens forming an image of some object such as a straight edge. Appearances depend on assumption as to the nature of the light by which the edge is illuminated. Fig. 3 shows the case of an edge in a self-luminous field. At the edge of the shadow the intensity is halved, and with a perfect lens it falls off within the shadow but gradually reaches a maximum in the illuminated part some little way from the edge. The changes take place in a very short distance, but by measuring photometrically the changes of intensity a curve could be drawn, and by comparing this with the theoretical curve the

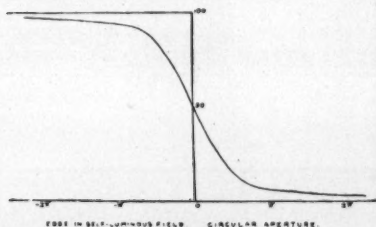


FIG. 3.

quality of the lens so far as defining power is concerned could be measured. One method of investigating the defining power is by examining the image of a series of black or white stripes formed by the lens, noting the sharpness of the edge. Mr. Hunter has recently devised an instrument for actually measuring the luminous intensity close up to the edge and plotting the curve, and this is used for examining defining power.

Or, again, take the image of a star. It is surrounded by diffraction rings (intensity curve, fig. 4), and the investigation of the shape of these rings is one method of testing the

quality of the lens used. Fig. 5 shows diffraction patterns for an annular aperture.

If a simple object gives complicated images like these, what will happen if the object is complex? If two stars or two lines of light are close together the rings and fringes may overlap so as still further to prevent recognition. How close together may two objects be and yet be distinguished by a lens or by the eye as separate? This leads us to consider the resolving power of a lens, which is distinct from perfection of definition.

Consider first two lines of light at a distance from a telescope with rectangular aperture. Let them be quite independent, at a distance ϵ apart, and d away from the telescope. The angular distance between them is ϵ/d . Each produces

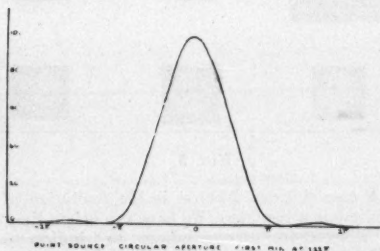


FIG. 4.

its own distribution of intensity in the view plane. We have two independent intensity curves superposed, and the resultant intensity is the sum giving another curve, fig. 6. What we see depends on the shape of this curve. If the first maximum of the second source coincides with the first minimum of the first source the resultant curve has two maxima with a 10 per cent. drop between. This is quite recognisable. The two lines are just resolved. This is Lord Ray-

leigh's criterion. For closer distances the drop is hardly noticeable. Professor Porter suggests as a definite limit the position in which the curve between the two is practically flat (see fig. 7). For a rectangular aperture the angular distance between the maximum and the first minimum is λ/a , and this must be not greater than ϵ/d . Thus, for resolution ϵ is not less than $\lambda d/a$ (Lord Rayleigh's experiments).

In the case of a star and circular aperture the formula is rather more complex, the principle is the same.

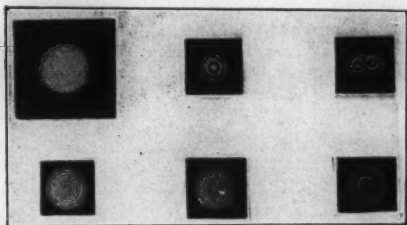


FIG. 5.

A case of great interest is the resolution of the lines in a spectrum. We have two bright lines in a spectrum corresponding to indices μ and $\mu + \delta\mu$. What are the conditions that they may appear separate? They will be dispersed or separated out on traversing the prism. We must find the angle between them. It can be shown that if the light fills the prism from its vertex to a point where its thickness is t , and if b is the breadth of the emergent pencil, then δD the angle between the lines is given by

$$\delta D = \frac{t \delta \mu}{b}$$

This is the angle between the two independent

linear sources already considered, and this must be greater than λ/a . Thus $\delta\mu$ must be greater than $\lambda b/at$. If the telescope lens uses the whole of the pencil from the prism b is equal to a , and the limit of resolution is given by λ/t . This shows us the importance of the length of path in the glass.

In a similar way we can discuss the resolving power of a grating; we find—

$$\frac{\delta\lambda}{\lambda} = \frac{\epsilon}{na} \cos \phi = \frac{1}{Nn}$$

where ϵ is the grating interval, n the order of the spectrum and ϕ the diffraction angle and N is the number of lines in the grating.

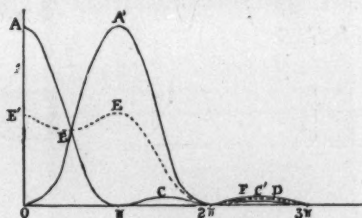


FIG. 6.

Proofs of these formulæ are given in the Appendix.

In these cases we are dealing with light from a distant source; the incident waves are plane, the lens is practically the O.G. of a telescope. The results are of interest to the astronomer or spectroscopist, to a less extent to the microscopist.

We will turn now to the case of the photographic lens as ordinarily used, or to that of a microscope when the object is near the lens and the waves reaching the lens are no longer plane.

The problem has been treated by Lord Ray-

leigh also in some simple cases, such as that of a grating placed in front of a convex lens which forms a real image of the grating at the conjugate focus. At first we deal with two luminous lines only, not a complete grating.

Two cases arise:—

(1) The lines behave as two independent self-luminous lines.

(2) The lines are illuminated by a train of plane waves either parallel to or oblique to their plane.

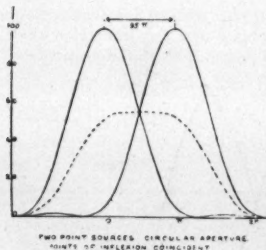


FIG. 7.

Our problem is to find the resulting intensity.

In the first case we have to find the intensity at each point of the image plane due to each line and add these up; in the second case the phase relations have to be considered, the light from one source will interfere with the light from the next, and Lord Rayleigh has drawn for two sources the curves (fig. 8) for the three cases (I.) in which the phases over the slits are in agreement, (II.) in which they are in disagreement, while (III.) gives the cases for two independent sources, assuming in each case that the maximum due to the second source coincides with the first minimum due to the first. The one maximum would be at 0, the other at $-\pi$.

In I. there is no sign of resolution, in II. it is complete, while in III., which resembles the telescope case, it is quite appreciable. Compare I. and II. with Mr. Conrady's results, in which he shows the doubling of the structure due to reversing the phase differences between the lines of a grating.

Moreover, it can be shown that if ϵ is the distance between the sources, and α the angular aperture, then for Case I. $\epsilon = \frac{1}{2} \lambda / \sin \alpha$ and resolution is possible with twice the closeness given by Abbe's theory.

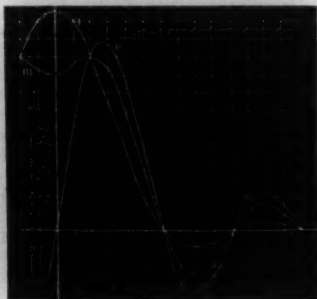


FIG. 8.

If we turn to the case of a grating, a large number of parallel lines, the same distinction as to the independence of the sources, or as to their being in phase relation, has to be made.

Take first the case when the grating consists of a number of independent line sources. In Lord Rayleigh's notation the distance between the geometrical images of the successive sources is represented by a quantity v . When v is large and the geometrical images well separated, we get resolution. As the images come closer and closer together, the structure gets less

distinct, until for a value corresponding to $\epsilon = \frac{1}{2} \lambda / \sin \alpha$, for which a pair of lines shows resolution, all semblance of structure has gone.

When the grating is illuminated by a parallel beam so that all the sources are in phase, the image in the image plane is shown to be a complete representation of the object so long as ϵ is considerable, but all signs of structure go when $\epsilon = \lambda / \sin \alpha$. This is Abbe's case, and his limit for resolution. It is twice the value for resolution for two independent sources. The mathematics show that the intensity can be represented by a series of terms given by the formula

$$I = \frac{\pi}{v} \left(1 + 2 \cos \frac{2\pi u}{v} + \dots + 2 \cos \frac{2\pi r \pi u}{v} + \dots \right)$$

the series continuing so long as $2\pi r$ is less than v , where u is proportional to the distance of the point at which I is considered from the centre of the field, and v to the distance between the geometrical images. As v is reduced and the images close up, the number of terms falls, and when v is less than 2π only the first term remains, and then, as we have said, $\epsilon = \lambda / \sin \alpha$. Each of these terms represents the effect of one of Abbe's side spectra. When only the first term remains the side spectra have all disappeared; there is no resolution.

A similar result follows if the lines, instead of all being in the same phase differ in phase by a constant. This state of things can be produced by a plane wave incident at a suitable angle γ , and the phase difference will be proportion to $\epsilon \sin \gamma$. The term involving r in the series of the r th spectrum will then vanish, as can be shown, if—

$$\epsilon (\sin \gamma \mp \sin \alpha) = \mp r \lambda$$

Resolution ceases when r is 1 and then—

$$\epsilon (\sin \gamma \mp \sin \alpha) = \mp \lambda$$

Thus resolution is correlated with the presence or absence of Abbe's diffraction spectra in the

way Mr. Beck is kindly going to demonstrate to us; and Abbe's criteria for the case of a grating of many lines are correct.

But we have already considered cases in which resolution is possible with smaller intervals than given by this theory, and it must not be applied to all problems indiscriminately. Thus, take the case of a narrow black band crossing a uniform field produced by a plane wave. Then Lord Rayleigh shows that the amplitude of vibration is, if the bar is small, nearly the same over the whole of its geometrical image, being given by the expression—

$$\frac{I_0 - I}{I_0} = \frac{2v}{\pi}$$

When I is the actual intensity I_0 the intensity at the centre if the bar were removed and v^* an angle proportional to the actual breadth of the bar.

If we assume that a drop of $1/8$ of the maximum intensity could be noticed by the eye, the bar will be quite visible if $(I_0 - I) / I_0$ is equal to $1/8$ or v equal to $\pi/16$.

Now for minimum visibility in the case of a grating we are to have v equal to 2π .

Thus the dark bar will be visible when its width is reduced to $1/32$ of the minimum grating visibility. Moreover, it can be shown that this illumination, I , extends, practically uniform, far beyond the geometrical image, so that the apparent width of a dark bar is quite illusory.

If the bar is on a self-luminous ground instead of one illuminated by a plane wave we have instead of the above

$$\frac{I_0 - I}{I_0} = \frac{v}{\pi}$$

the bar is only half as visible as before, while the smallest resolving grating interval is also

* The quantity v is equal to $2a$ in Lord Rayleigh's paper.

half what it was in the first case. At the same time the apparent breadth of the bar is greatly increased as in the former case.

Some figures as to resolving power may be of interest.

For an objective of N.A. 1.4, the limiting grating interval * which can be resolved under "critical" illumination is given by—

$$\epsilon = \frac{1}{2} \left(\frac{\lambda}{\mu \sin \alpha} \right) = \frac{1}{2} \left(\frac{.5 \text{ micron}}{1.4} \right) \text{ (taking } \lambda = .5 \text{)}$$

=.18 of a micron.

(141,000 lines to the inch).

For $\lambda = .4$ * = .14 of a micron.

(178,000 lines to the inch).

For $\lambda = .275$ * = .10 of a micron.

(254,000 lines to the inch).

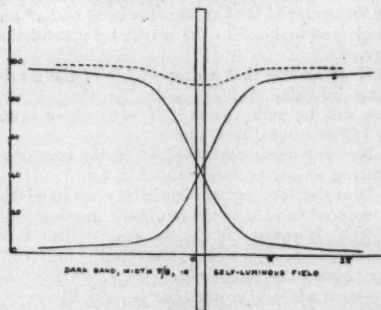
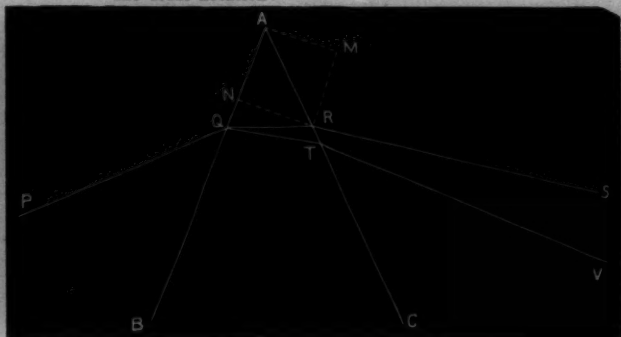


FIG. 9.

With the same objective, and with "critical" light of W.L. .5 a single dark bar of width .025 of a micron will be distinguishable as a band of at least 4 times its actual breadth, over which the illumination is 10 per cent. less than in the rest of the field (fig. 9. The dotted curve shows the distribution of light intensity in the image).

Appendix.

In the figure, P Q R S is one ray of index μ , P Q T V an adjacent one of index $\mu + \delta\mu$. R M is perpendicular to R S, R N to A B. Then with the usual notation:—



$$D = \phi + \psi - \epsilon$$

$$\epsilon = \phi' + \psi'$$

$$\sin \phi = \mu \sin \phi' \quad \sin \psi = \mu \sin \psi'$$

$$\delta D = \delta \psi, \text{ since } \phi \text{ does not alter;}$$

$$0 = \delta \phi' + \delta \psi'$$

$$0 = \sin \phi' \delta \mu + \mu \cos \phi' \delta \phi'$$

$$\therefore \sin \phi' \delta \mu = -\mu \cos \phi' \delta \phi' = +\mu \cos \phi' \delta \psi'$$

$$\cos \psi \delta \psi = \delta \mu \sin \psi' + \mu \cos \psi' \delta \psi'$$

$$= \delta \mu \left\{ \sin \psi' + \frac{\sin \phi'}{\cos \phi'} \cos \psi' \right\}$$

$$= \frac{\sin \epsilon}{\cos \phi'} \delta \mu$$

$$RN = AR \sin \epsilon = QR \cos \phi'$$

$$\therefore \frac{\sin \epsilon}{\cos \phi'} = \frac{QR}{AR}$$

$$\cos \psi = \frac{RM}{AR}$$

$$\begin{aligned}
 \therefore \delta\psi &= \delta\mu \frac{\sin \epsilon}{\cos \phi'} \frac{1}{\cos \psi} \\
 &= \delta\mu \frac{QR}{AR} \times \frac{AR}{RM} \\
 &= \delta\mu \cdot \frac{QR}{RM} \\
 &= \frac{t \delta\mu}{b}
 \end{aligned}$$

Again, for a grating we have, if ϕ is the angle of diffraction, n the number of the spectrum, and ϵ the grating interval,

$$\begin{aligned}
 \epsilon \sin \phi &= n\lambda. \\
 \text{Hence } \epsilon \cos \phi \delta\phi &= n \delta\lambda \\
 \text{and } \delta\phi &= \frac{n \delta\lambda}{\epsilon \cos \phi}
 \end{aligned}$$

But for resolution $\delta\phi$ is to be greater than $\frac{\lambda}{a}$

$$\begin{aligned}
 \text{Hence } \frac{n \delta\lambda}{\epsilon \cos \phi} &\text{ is greater than } \frac{\lambda}{a} \\
 \text{or } \frac{\delta\lambda}{\lambda} &\text{ is greater than } \frac{\epsilon \cos \phi}{na}
 \end{aligned}$$

But if N is the total number of lines, $N\epsilon$ is the total breadth of the grating, and a being the breadth of the pencil, we have $\cos \phi = \frac{a}{N\epsilon}$

$$\text{Thus } \frac{\epsilon \cos \phi}{a} = \frac{1}{N}$$

$$\text{Hence for resolution } \frac{\delta\lambda}{\lambda} \text{ is greater than } \frac{1}{Nn}$$

These formulæ are proved in Schuster's Optics.

ON COLOUR VISION.

BY PROF. G. J. BURCH, M.A., D.Sc., F.R.S.

*Lecture delivered before the Optical Society on
April 7th, 1908.*

Science has become so vast a subject in these days that no self-respecting man, even in one branch of science, will undertake to know all the elementary parts of it. The things that we really do interest ourselves about with regard to our neighbour's hobby are his latest discoveries. And for this reason I, personally, always feel thankful to a lecturer who will condescend to spend the first five minutes in going over the A B C of what he is going to talk about. Of course, I ought to have learnt it all at school, and very likely I did, but it helps me to understand and to enjoy a lecture if the lecturer will, at the beginning, treat me as if I knew nothing at all about the subject. Therefore, I am going to ask your indulgence while I remind you of the fundamental facts of "Colour Vision."

First of all, I would ask to have put upon the screen a diagram of the eye. The eye, as you know, is like a photographic camera; it has a compound lens, consisting of the cornea, the aqueous humour, the lens itself, and the vitreous humour. It is provided with what is, in point of fact, the original of the iris diaphragm—namely, the pupil. But the eye goes one better than the photographic camera, in that the adjustable stop it possesses is automatic, contracting of itself when the light is excessive, and expanding of itself when the light is not sufficient. Then, too, there are muscles attached to the lens, which, by their action, partly alter its shape and

partly alter its position. In the case of fishes the focus is principally attained by altering the position of the lens. These muscles have the effect of focusing the image upon the sensitive surface, which is known as the retina. But whereas with a camera you have to put in a fresh plate, and when you have taken a photograph you takē the plate out, and pour the developer upon it, and carry away the picture, in the case of the eye, the images are not fixed permanently, although they are fixed for a short time, upon the retina, but are telegraphed, as it were, through the optic nerve to the brain, where they undergo a process of sorting, and comparing, and arranging, and recording, by a somewhat capricious method of selection, until they are stored up in the memory, to be brought out thence as we may bring out our photographic picture, when they are required, or sometimes when we can lay our hands upon them. So that, after all, the eye is very much like a photographic camera.

But to-night I am concerned, not so much with the optical system of the eye, nor even with the microscopic construction of the retina, with its rods and cones, but with the functions of the retina. And there are one or two things about it which I should recall to your minds. First, there are two special points of peculiar and particular interest on the retina—the blind spot and the yellow spot. The point where the optic nerve goes into the eye constitutes the blind spot; because the optic nerve is no more capable of receiving a sensation than a telegraphic wire is capable of receiving a message. It merely transmits the sensation to the organ which makes you aware of it. The nerve itself is absolutely insensitive to light. There is another thing which gave me the greatest astonishment when I first learnt it. The nerve which transmits the images to the brain comes through from the back of the eye, and spreads out its branches all over the front of the sensitive surface that

receives the picture. One would naturally suppose that the nerves would be at the back of the rods and cones. The latter receive the light—they give the sensation—and one would expect that the nerve should lie behind the mechanism for receiving the impulses of light. But no; it spreads over the front of it. The reason why this is so is bound up in the history of life upon this world, and we could not explain it without going into the long history of the way in which the sense of vision has developed. The nerve, then, comes through the eye, spreads over the inner surface of it, and from the organs at the ends of the tiniest branchlets of the nerve is the picture transmitted. And so it comes about that the part where the nerve fibres are bunched together in order to go through to the brain is absolutely blind. Of course, you know the simple experiment for demonstrating that. You take the cover of a book, you lay upon it two little chips of white paper, the smaller the better, side by side, about three inches apart, and hold it about a foot from your eyes. Now shut the right eye, and look at the right-hand piece of paper, or shut the left eye, and look at the left-hand piece. By moving the book-cover a little nearer, or a little farther off, you will find the position where the spot you are not looking at vanishes. If you pass your finger round it, you can see your finger going all the way round, but you cannot see the spot, because when your eye is in that position, the image of one of the spots falls upon the nerve where it goes through the retina from the front to the back, and that part is blind. It is so far blind that you may put two lighted candles side by side, and look at one of them with one eye shut, and when you get them at the proper distance the other flame will disappear. You may, if you have sufficient command of your muscles, place a candle side by side with an arc lamp, and the arc lamp will disappear. Or you may even—still if you have sufficient command of your muscles, and with

the aid of a piece of looking-glass—perform the same experiment with the image of the sun itself, and you will not see the sun, because that part of your eye is absolutely stone-blind.

The other interesting part of the retina is the yellow spot. The retina there is thinner than it is anywhere else. It is the most sensitive, and the most accurate part. In point of fact, there is no definition worth speaking of in any other part of your eye. You cannot read with any other part but the yellow spot. It does all the reading, and is consequently generally in a state of chronic fatigue.

The special sensitiveness and the peculiar properties of the yellow spot may be demonstrated by the following experiment. Take a flat bottle—a medicine-bottle will do very well—and fill it with a solution of chrome alum. You must find out the proper strength by trial. The colour of the solution is a dirty bluish purple. Stand with your back to the window, on a day when there is plenty of blue sky, cover one eye, hold the bottle close to the other, and wait a minute or two. The purpose of that is to rest your eye. Then suddenly swing round, and look up through the bottle at the blue sky. You will see a patch—with some people it is oval—with myself it is round—a patch of a sort of purplish-crimson colour. The reason is because, as I said, the yellow spot does all the work of the eye, and consequently is in a state of chronic fatigue, and therefore, when it gets a holiday, enjoys its holiday, and comes back from it not fatigued and *blasé*, but very much refreshed, just as do the people who perform the work of the world. When they return from a holiday, they are in good spirits, and not at all tired, and it is just so with the yellow spot. It is now capable of seeing the crimson light in the sky which, to the rest of the eye, is overpowered by the blue. That is one way of demonstrating the existence of the yellow spot. Sometimes at night, when you are trying to go to sleep, you may see floating about

before you—floating away when you look at it, yet coming into view when least expected—a little group of bright stars, sometimes yellow, sometimes green, sometimes orange, floating about in the field of vision. Well, that is somehow or another, in a way that no physiologist has yet explained, connected with some functional activity of the yellow spot. Those are the principal ways of evidencing the existence of the yellow spot.

We now come to the theories of colour vision. One may say that the science of colour vision began with the discovery of Sir Isaac Newton, that light can be decomposed into rays of different frangibility and of different colour. Sir Isaac Newton said that these colours were in all seven—red, orange, yellow, green, blue, indigo; and violet. Later on, Wollaston taught that there are four colour sensations. He pointed out that the seven mentioned by Sir Isaac Newton differed somewhat in importance—in fact, Newton himself speaks of some of these colours appearing to be, as it were, transitional colours from one to another. Wollaston considered that yellow and orange were both transitional colours between red and green. He thought that blue was a separate colour, distinct from violet. But a man named Wünsch, who had made experiments by projecting upon the screen different colours produced by means of a prism, and comparing them with the seven colours of the spectrum, made the statement that if you mix together green and violet you produce the effect of blue. So it came to pass that with these data before him as regards colour-mixing, Thomas Young, about the year 1800, gathering together all that was known up to that time concerning colour vision, put it into the form known as "Young's Theory." That theory is based upon three fundamental facts. The first is that we possess a limited number of colour sensations—three or four. The second is that, in addition to producing sensation, the light of any given col-

our falling upon the eye temporarily diminishes its sensitiveness to that colour, and Thomas Young compared this process to muscular fatigue. The third fact on which his theory is based is that the sensation evoked by light of any given colour persists after that light has ceased to act, and only gradually dies away. In the year 1761 a very wonderful paper was written by a man named Scherffer, describing the phenomena of fatigue. He showed how, when the eye has been exposed to light of any given colour, it becomes less sensitive to that colour, so that if one looks afterwards at light containing several colours, then from those several colours there is subtracted light of the colour to which the eye has been exposed. For instance, supposing I expose the eye to red light, and afterwards look at white light, then the white light will appear to me no longer white, but white minus red, and if the four sensations are red, blue, green and violet, then white would appear, after exposing the eye to red light, to be a mixture of green, blue, and violet, which would look blue. If, on the other hand, I expose the eye to blue light, then I shall see all the colours that are left out of white after subtracting the blue from it. That is called the negative after-effect. The next discovery was made in 1786, by Robert Waring Darwin, one of the great family of Darwins, who showed that not only is the eye fatigued by being exposed to light of a given colour, but also that the sensation of that colour persists for some time after the light has been taken away. That is called the positive after-effect. So that you see Scherffer discovered the negative after-effect, and Darwin discovered the positive after-effect. Thomas Young's theory is based upon these facts, and Thomas Young supposes that we have in the eye nerves—or, as we should say now, end-organs, because we now know that the nerve itself is not sensitive to light—we have in the eye end-organs belonging to the nerves, some of which respond specially

to red, some specially to green, and some specially to blue, or violet, or both. And Thomas Young, putting together all the experimental results of his contemporaries, was obliged to agree that although, for many reasons, blue seemed to be a separate colour-sensation, yet, in the absence of positive proof of this, he was bound to accept the experiments of Wünsch, and agree that there were three colour-sensations—red, green, and violet. And he was particular to note that although the red sensation is excited most strongly by red light, yet it is excited a little by blue light, and similarly green is excited a little by some of the red light, and a little by the violet. The violet also, he thought, might be excited slightly by the green, still less by the orange, and least of all by the red—but most of all by the violet; and according to Thomas Young, white light is due to the equal excitation of all these three sensations simultaneously.

About half a century after the time of Thomas Young, Helmholtz took up the subject independently, and arrived at a conclusion almost exactly the same. Maxwell also supposed that we have three colour-sensations, but considered them to be red, green, and blue. It seems extremely probable that Maxwell was partly violet-blind, so that violet was a much less important colour to him than to Helmholtz, and that may have been why he gave preference to blue as a fundamental colour-sensation. Another scheme of colour-sensations was given by König. You notice his idea of red is that it begins about the B line, that it reaches a maximum in the yellow, that it then comes down to a minimum between green and blue, and rises again to a second maximum as you get into the violet. He makes the green begin by the line C, come to a maximum in the yellow, and fall to a minimum—to zero—before getting to the violet. König gives much more importance to blue as a colour than he does to either of the other two. It would

seem that to him red was not so important a colour relatively as it is to many people.

While the next slide is exhibited, I would like to have the room as dark as it can be. There will be put upon the screen a bright red object, and I want you to look quite steadily at the little notch near the middle of it until you feel you cannot look any longer and must look at something else, and then you will see the effect I want you to see in a very striking manner. While you are looking I will tell you the reason why I show this. It is because some 25 or 30 years later than the time of Helmholtz, another theory was put forward by Hering. All the theories I have hitherto spoken of are varieties of one theory, but this of Hering is totally different. It is based upon the idea of contrast, and it says that we have in our eyes several chemical substances, of which the most important is the black-white substance. This, while it is being used up, causes the sensation of white, and while it is being secreted, the sensation of black. Then he says we have another substance, which gives the two sensations of red and of green—while it is being secreted it gives one sensation, while it is being used it gives the other. Similarly, a third substance is supposed to give the sensations of yellow and blue. All this while you have been using up the substance that gives the red sensation, and now I am ready for the experiment. I push from behind the slide the red glass, and substitute colourless ground glass. The figure looks green to some and blue to others, and to a few pink. If we had had the room perfectly dark, it would have looked blue to all of you. The reason is, according to Hering, that we have been using up the red substance, and now it is being secreted again, giving the sensation of green. According to Young and Helmholtz, the reason is that you have been fatiguing the red sensation, and when you look afterwards at the white light, you see

all the blue, all the green, and all the violet of it, but only a small proportion of the red.

When I went to Oxford, I found this theory of Hering was holding the field; the theory of Young and Helmholtz was considered refuted, and I should have believed the theory of Hering, as in duty bound, but for a curious little circumstance that happened when I was quite a young man, little more than a boy. I was amusing myself with a telescope, and, an eclipse having been announced for a few months later, was preparing my telescope to observe the sun, and was trying various pieces of red glass for the purpose. My red glass was not dark enough, but young people are not always very cautious, and I did examine the sun through it. I suppose the telescope magnified about 40 times, so that the sun more than filled the entire field of the eye-piece, and when I took my eye away I was horrified and astonished (it was out of doors), on looking at the flower-beds, to see the geraniums absolutely black. The leaves looked green, but the flowers appeared black.

The next slide I am going to show will illustrate what I saw. It is a photograph of a rose, which I have coloured to represent the colour as it appeared to the normal eye—a bright pink. In the next slide, you will see how it appeared to me after I had looked through this telescope—a blue colour. The roses looked blue, the geraniums looked black, and the yellow flowers green, while the green leaves looked their ordinary colour. You may imagine that it was a very startling experience, and it led me to make some more experiments on the same lines. I tried different coloured lights. It was difficult to get coloured glass that would serve for green or blue, and the spectroscope showed that such colours were not pure. After a while I tried experiments with such prisms as I had in those days, and ascertained to my own satisfaction that it was possible temporarily to blind the eye to any one of the principal colours. Of

course, only knowing the theory of Young, I thought it was all in complete accordance with that theory, and there was nothing more to be done. But when I came to Oxford, and learnt about the theory of Hering, it seemed advisable to repeat these experiments. I did so, with the aid of some of the apparatus presented to the University by the Dowager Duchess of Marlborough. Broadly speaking, this was the result. If you look at a very brilliant red light—the red of the spectrum anywhere about line B, and then look at a spectrum of moderate intensity, you will find that the violet, the blue, and the green, are perfectly visible, but the green runs right into the yellow up to about the C line, and there the spectrum ends. There is no yellow—it is all green. If you blind the eye to green, the effect is that you can see blue and violet, and you can see red, but the red and the blue meet—there is no green between them. If you blind your eye to blue and violet, then the spectrum ends with green about half-way between F and G. The effect, in the case of red blindness, lasts about 10 minutes; in the case of green blindness, about half an hour; while if you blind your eye with blue, the effect lasts fully an hour. In the case of violet, a very intense light is required, but the effect lasts fully two hours before it has wholly gone. Now suppose you blind your eye with yellow light, what is the result? The red has gone, the green has gone; the spectrum begins near the B lines with blue, and goes on to violet. And although the effect of the blinding light may have been so strong that you cannot see to read a book, you can still see the other spectral colours to which you have not blinded yourself. That is to say, when red-blind you can see violet, blue, and green; when green-blind, you can see red, blue and violet; when violet-and-blue-blind, you can see the faintest red, as well as green. Each colour sensation is absolutely independent of the rest.

This enabled me, to my own satisfaction, to settle one or two questions. Thomas Young had originally stated—following Wollaston—that there were four colour sensations—red, blue, green, and violet. Thomas Young gave up blue, because Wunsch said that green and violet mixed made blue. I was able to settle that question, because I found that you can blind yourself to violet without affecting the blue, and you can also blind yourself to green without affecting the blue, and therefore those two cannot produce blue. Then, again, Maxwell said that violet is produced by a mixture of blue and red. If that were so, then if you blind yourself to red it ought to reduce the violet and leave only the blue. But it does not—you are still able to see the violet.

The next slides will show what the spectrum looks like during the process of blinding the eye. The first represents the spectrum from the E line to *b*. Imagine that it is a spectrum of very wide dispersion, and that you have got the full strength of the sunlight on it, so that it is almost painfully bright to look at. As you look at it it begins to get white. That is the stage at which most experimenters have stopped. But if you keep on looking at it, the colour comes in again, and when the green is entirely gone, the red and blue meet. And if you look towards the blue side of the spectroscope the red appears to encroach upon it, while if you look towards the red side, then the blue appears to surge up and go over towards the red. If, on the other hand, you try that part of the spectrum near the F line, where the blue and green meet, you may look at it as long as you like, but it will never change; you cannot get any other colours than those two. Now, suppose you turn to the blue part of the spectrum, and that you look at it steadily till your eyes ache. It will first lose colour and become white, then the green will begin to come in at one side, and the violet at the other, until they meet in the middle

of what was the blue. The explanation I give is that the colour which is most strongly excited is most powerfully fatigued, and it is possible to become blind to that colour without affecting the others. I think that these experiments are completely contrary to the theory of Hering, and that they corroborate the original theory of Thomas Young. I believe that this Society is interested in the work of Thomas Young, and I am very pleased to say that in my opinion his theory was not only the true theory, but was even more true than he imagined—that is to say, his original theory of four colours, red, green, blue, and violet (which he gave up in consequence of Wünsch's experiments) is the true one.

When I first published these experiments, the objection was raised that it is really a pathological condition which I produced by this intensely brilliant light, and that therefore it could not be said that the experiments were perfectly fair. My answer was that you can perform all these experiments with moonlight, the only difference being that you must stay in darkness long enough to allow the effect of previous daylight, or artificial light, to subside completely from your eye.

Now, with regard to "colour blindness." I have made use of this method for testing colour-sensations. I do not expose the eyes of my victims to the intense light that I used in my own case—and let me say here that I do not recommend that any of you should do this. I repeated the experiments a good many times with impunity, but the last time I did so, for the purpose of the illustrations that accompany my paper in the *Philosophical Transactions*, I found that after the experiment there was a great triangular patch of total red-blindness on the left-hand side of the field of one eye, and that condition of things lasted for very nearly two years. It has quite died out now, and the eye is normal again, but Sir

George Stokes told me that his eye was permanently damaged by a somewhat similar experiment, and the oculist who examined his eye informed me that you could see the effect on the retina of the burning of the intense light to which it had been exposed. So be cautious, and do not let me incite any of you to perform these experiments without fully realising that by so doing you will risk your eyesight. But I use the same method for testing people's eyesight in this way. Colours such as you look at habitually do produce a very considerable and measurable amount of colour-blindness. For instance, if you attend a cricket match, you will probably be more than three parts green-blind when you come in afterwards, as the effect of looking at the green grass. You may make yourself quite half red-blind if you sit in a room papered with crimson paper, in the sunlight. I use these facts in testing people. I make them look at the red of the spectrum, using a quite moderate intensity, and, after 30 seconds' exposure to that colour, they turn the screws of the spectroscope so as to traverse the spectrum across the field, and stop where the colour changes from red to green. Then I let them look at the green, with the ordinary intensity you would use for examining the spectrum. When they have looked for 30 seconds, I make them find where blue begins, and when they have looked at blue for 30 seconds I let them find violet. Then I make them determine how far they can trace the violet to the end of the spectrum. Having done that, I make them look at the violet for 30 seconds, and find where the blue begins. After exposure to blue they find where green begins, and after looking at green they seek the beginning of red. On comparing the results, it is found that they have called the same part of the spectrum red or green, according as they have been looking at green or red. In other words, the red and the green sensations overlap. So do

the blue and the green, and also the violet and the blue. By this method the extent of the overlap can be measured, and the middle point of each overlap may be taken as the natural boundary between the adjacent colour sensations. To the normal sighted these boundaries occur about D for green and red, near F for green and blue, and about G for blue and violet.

A man who is red-blind will not be able to see very far into the red, and he will find no change at D—there may be a change of intensity, but not a change of quality, though he will very likely tell you there is a change if he has been taught to use the spectroscope, and will say that one part is red and the other yellow or green. By the method I have described, his red-blindness may be detected. Let him point out, without previously fatiguing the eye, where what he calls "yellow" begins. Then, after fatiguing him with red light, let him find the same "yellow" or green, or whatever he calls it. If he really possesses a sensation of red as well as green, he will find the change of colour nearer to the red, but if he possesses no red sensation, the change will appear to have moved farther into the green, because it is not caused by a different colour-sensation, but is simply the brighter part of the same colour, and it moves further away because he is fatigued. A man who is colour-blind to green will find no change of colour at D, but a very sudden change at *b* in the middle of the green where normal people see no change, or very little. It is necessary to remember that in dealing with a colour-blind man, you are dealing with a man who talks another language. I have some examples here of skeins of wool, which have been called a good match by the colour blind. One is bright red matched with dark green. Another is violet, matched with a sort of reddish-brown. For the purpose of detecting colour-blindness, these skeins of

wool are the quickest and the best, but they need some training in their use. Just as the doctor learns how to diagnose diseases, so you have to learn to diagnose cases of colour-blindness—you have to watch the person who is doing it to see if he is really making mistakes, or if he is trying to deceive you. Remember that there are two classes of persons who come to have their sight tested—there are those who want not to be found out colour-blind, and there are those who, knowing themselves to be colour-blind, are interested scientifically in it. Now, for the second class of people the spectroscope test is the surest and quickest you can have, but for the first class of people I think a good selection of wool skeins is far the best. There should be at least two samples of each colour; and you want a great many different delicate tints as well as the brighter ones. The slide I am now showing is a very curious case—a piece of green glass and a piece of purple glass, given to me by a person who was green-blind, and who said that those two colours were exactly alike. Green-blindness is very much rarer than red-blindness. On an average about 4 per cent. of men are colour-blind, and only about .4 per cent of women.

Very occasionally one meets with people in whom the blue colour-sensation is completely absent. In a very great many people it is largely absent, and I believe that is one reason why so many writers have said that there is no separate sensation of blue. I have come across one person in whom the violet sensation is completely absent. In the spectrum of the electric arc, there is a group of lines between the blue and the extreme violet fairly close together. In one of the cases I examined, the person could see all the lines in the group up to $\lambda 4160$, whereas I could see lines beyond as far as H and K. To him the spectrum ended absolutely and completely at that point, and I found by other measurements that

in his case the sensation of violet was completely lacking, while he had those of red, green, and blue.

There is only one other thing I have to mention. The relative sensitiveness of the eye to different colours is continually varying to an enormous extent. I told you how, if you had been playing cricket, you would find yourself almost completely green-blind because of the green of the grass. I find that it is necessary to stay at least two hours in darkness in order that the eye may recover completely from all the effects of previous light, and even that, in many cases, will not be sufficient. Two hours is the shortest period in which it is possible to get rid of all the after-images. I found, by actual experiment, that the eye, when it is completely rested, is from 15 to 20 times as sensitive to red as it was before—the red sensation increases in power 15 to 20 times, but the blue sensation increases in power from 1,000 to 1,100 times, so that you are able to see light 1,000 times as weak after you have been two hours in darkness, as you can in bright daylight. That means that when you go out of the bright light into a room which is partially dark, your estimate of colours alters rapidly and very considerably, so that a colour which appeared to be bright red may, on going into a dark room, seem perfectly white. Let me conclude by suggesting this very simple experiment. Some bright, sunshiny day, when you are out in your garden, pluck a petal from a scarlet geranium, and lay it upon your hat, which for that purpose must be a black one. Carry it with you into the house, and go into the darkest corner you can find—into your cellar. As you get into the gloom, looking at this brilliant scarlet petal, it will change from scarlet to orange, from orange to yellow, and the last you see of it it will be quite white. That is simply because the sensitiveness of your eye alters to different colours in different

proportions, and the very small amount of blue, green, and violet reflected by the scarlet geranium petal is sufficient, as the eye becomes more sensitive, to mix with the red and to produce white.

I feel that I have taken up a great deal more time than I ought, but still I have not by any means exhausted the subject, and my excuse must be that there is a very great deal to be said upon it.

THE OPTICS OF LIGHT PROJECTION.

BY VAL. H. MACKINNEY.

Communicated on May 12th, 1908.

BEFORE dealing with the subject matter of this article I must mention the fact that I am very much indebted to the "Manual of Military Electric Lighting," and to an article on the "Photometry of Projectors," by M. Fèry,[†] an abstract of which appears in Vol. 23, No. 110, of the 'Journal of the Institution of Electrical Engineers,' for enabling me to deal with the subject in so concise a manner. I wish it to be clearly understood that to M. Fèry and to the writer of the said Manual, all credit is due, all that I have done in this article is to collect various matter together, rearrange it, and endeavour to explain points which to me have seemed at first sight rather ambiguous.

In dealing with this subject it is as well to bear in mind first, that the amount of light falling normally upon a screen (illumination) varies inversely as the square of the distance of the screen from the source.

This may be expressed as $I = B/d^2$, where B is the total brilliancy of the luminous source for the particular direction (candle-power, not intrinsic brilliancy). (1)

Secondly, that the amount of light falling obliquely upon a surface varies as the cosine of the angle of turning of the surface from the normal position.

We may, therefore, write $I = B/d^2 \cos. \theta$, where θ is the angle of turning of the surface from the normal position. (2)

Thirdly, that the quantity of light ϕ received

by a surface is given by the following equation, due to Lambert,

$$\phi = B_i \frac{A_1 A_2}{d^2} \cos \theta_1 \cos \theta_2, \text{ where } B_i \text{ is the}$$

mean intrinsic brilliancy (mean brilliancy per unit area) of the luminous source for the particular direction, A_1 and A_2 the areas of the luminous source and receiving screen, respectively (plane surfaces are assumed), and θ_1 and θ_2 the angles made by the normals to the luminous and receiving surfaces with the line joining the centres of the two surfaces. (3)

It is as well, too, to bear in mind that these formulæ are only correct providing the dimensions of the luminous source are small as compared with the distance d .

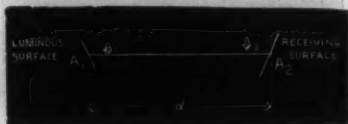


FIG. 1.

In practice we may wish to project light from a luminous source that is in no way under our control—the sun, for instance. Under such circumstances we are practically limited to the use of a plane mirror. A plane mirror mounted for flashlight signalling, and intended in practice to be employed in conjunction with sunlight, is termed a heliograph.

In fig. 2 let S represent the sun, H the heliograph, and R the receiving station. Now, in projecting the sunlight, by means of the heliograph, to the distant receiving station R , it is of the utmost importance that we should know, first, what the intensity of illumination at the receiving station does, and does not, depend upon; secondly, what the area illuminated at

the receiving station does, and does not, depend upon, and thirdly, what the shape of the area illuminated at the receiving station does, and does not, depend upon.

Let us consider, then, in the first instance, this simple case of the projection of sunlight by the heliograph mirror. We cannot, of course, take into account the absorption of light by the atmosphere, as this is by no means constant; it is as well to bear in mind, too, that in practice the diameter of the heliograph mirror is small in comparison with its distance from the luminous source, even if the luminous source

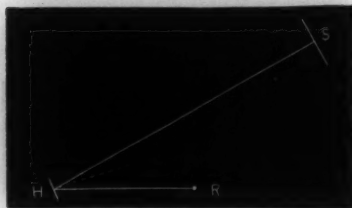


FIG. 2.

is an artificial one. Moreover, the plane of observation, in other words, the receiving station, is always at some considerable distance from the heliograph (signalling station). Assuming such conditions we may summarise our results as follows:—

Intensity of Illumination at Receiving Station.

Varies directly as the mean intrinsic brilliancy of the luminous source.

Varies directly as the area of the plane mirror.

Varies directly as the cosine of the angle of turning of the mirror from its position directly facing the receiving station.

Varies inversely as the square of the distance

of the mirror (heliograph) from the plane of observation (receiving station). Refer remarks made later.

Does not depend upon the total brilliancy of the luminous source, in other words, upon the size or shape of the luminous source.

Does not depend upon the obliquity of the luminous source to the central incident path to the mirror.

Does not depend upon the distance of the luminous source from the mirror (neglecting absorption).

Does not depend upon the shape of the plane mirror.

Area Illuminated at Receiving Station.

Varies directly as the area of the source of light.

Varies directly as the cosine of the angle of turning of the luminous source from the plane perpendicular to the central incident path to the mirror.

Varies directly as the square of the distance from the mirror to the receiving station.

Varies inversely as the square of the distance from the luminous source to the mirror.

Does not depend upon the intrinsic brilliancy of the luminous source.

Does not depend upon the shape of the plane mirror.

Does not depend upon the shape of the luminous source.

Does not depend (for all practical purposes) upon the size of the plane mirror.

Does not depend (for all practical purposes) upon the obliquity of the mirror to its position directly facing the receiving station.

Shape of Area Illuminated at Receiving Station.

Depends upon the size of the source of light, and resembles the foreshortened view as would be seen from behind the mirror. It must, therefore, depend also upon the obliquity of the prin-

front nodal point of the eye, we are not even employing the total area of one end-organ of vision (a cone in this case) as the receiving screen for our image. Hence, it is only reasonable to suppose that the nerve-stimuli set up vary directly with the size of the image when the image is smaller than the area of one cone, and, consequently, that the star of greater magnitude appears brighter than the one of lesser magnitude.

From the above figure (3) let us consider this matter more fully. Let L represent the adjustable crystalline lens of the human eye, and S the plane of the receiving screen, the retina. Let $A_1 B_1$ be the image of the luminous object $A B$ at a distance $A O$ from the lens. Consider first a luminous object $A C$ of double the diameter (four times the area) occupying the same position, let the mean intrinsic brilliancy be the same as before. The image will obviously be $A_1 C_1$, and its area will be four times the area of the image $A_1 B_1$; hence, the mean intrinsic brilliancy of the image is unaltered, and consequently the nerve-stimuli set in motion for each individual cone is the same in the two cases. Therefore, the intensity of illumination at the receiving station (the retina) does not depend upon the size of the luminous source, providing that the mean intrinsic brilliancy remains unaltered. Secondly, let us consider the luminous object $A B$ in the position $E D$, where $D O$ equals $2 A O$. The image will now be $D_1 E_1$, and in area it will be just one quarter that of $A_1 B_1$. In the image $D_1 E_1$ we have a reduction in the mean intrinsic brilliancy owing to the luminous object having been placed further from the lens. If the distance from the lens is doubled, then, from the law of inverse squares, the mean intrinsic brilliancy of the image must be a quarter of its original value, with luminous object in the position $A B$. But the area covered by the image is now just one quarter of its original value, therefore, surely, for this

reason the mean intrinsic brilliancy of the image must be four times its previous value. In other words, the two effects exactly counter-balance one another. Therefore, again, we may say that the mean intrinsic brilliancy of the image remains unaltered, and, consequently, the nerve-stimuli set in motion for each individual cone is the same as before. When, however, we are dealing with images whose size enables them to be received well within the area of one cone we must realise that the nerve-stimuli set in motion for the one cone is different to the maximum value, and, consequently, that under such circumstances we cannot say that the intensity of illumination at the receiving station, although still constant, produces the same visual effect.

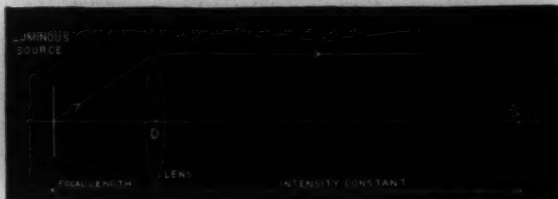


FIG. 4.

In the case of the projection of sunlight, or some other powerful distant luminous source, by the heliograph mirror, it is to be noticed that to an eye at the receiving station the light received by it, although actually diverging from the distant point, might be considered as a parallel beam (pencil). If the mean intrinsic brilliancy of the luminous source is very great and the aperture of the pupil of the eye very small, in comparison with the size of the luminous source, then, neglecting absorption, we might almost say that the beam of light is of constant intensity. The point that it is desired

to bring out here will be made clear if a single ray (path of a wave) is thought of. Likewise, the intensity of illumination will be constant if we have a number of parallel rays, in other words, if we have plane waves of light falling upon our receiving screen. This condition will, for all practical purposes, be satisfied when the naked eye views the sun or some other intensely luminous source placed at a great distance away.

Also, the condition here mentioned is satisfied, within certain limits, when light diverging from a point is converted into a parallel beam by means of a mirror or lens. But, unfortunately,

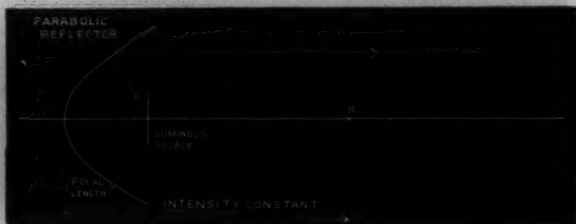


FIG. 5 (NEGLECTING DIRECT LIGHT).

in practice, our artificial sources of illumination are never of negligible size, so that we cannot assume always, either that the beam (approx. parallel) is of constant intensity, or that we may employ the law of inverse squares with exactness.

It has been demonstrated, however, by M. Féry (ref. cit.) that for projectors, signalling apparatus, etc., where, approximately, parallel beams of light are produced, the intensity remains—for all practical purposes—constant up to a point x (to be calculated hereafter) from the reflector or refractor, and that from this point onwards the law of inverse squares may be employed with considerable accuracy.

That such a condition should exist and hold good in practice, providing the photometrical screen is small in comparison with the aperture of the reflector or refractor, is made clear by the following diagrams.

From these diagrams (4 and 5) it will be seen that the point x is given by the intersection of the two refracted paths originally proceeding from the limits of the luminous source.

These two diagrams (6 and 7) show clearly, first, why the illumination on the photometer

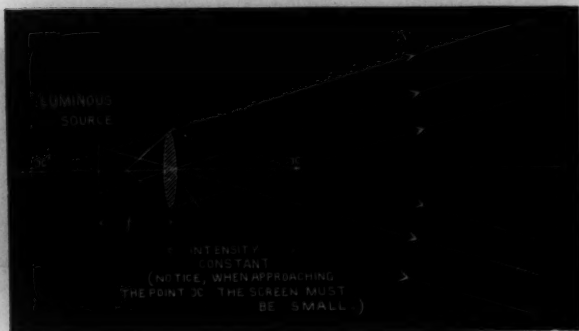


FIG. 6.

screen, when it is outside the point x , should vary inversely as the square of the distance (law of inverse squares); and, secondly, why (when observations are being taken) all measurements should be made from the reflecting or refracting surface.

It often happens that chromatic, and sometimes spherical, aberration prevents the results indicated from being obtained to a very great degree of accuracy. This fact should be borne in mind, and when possible allowed for.

For projectors and similar pieces of appara-

tus, it is usual to employ the crater of an electric arc as the luminous source. The luminous source, therefore, is comparatively small, and, consequently, the point x in such cases is at a considerable distance from the reflecting or refracting system.

In order, then, to determine, experimentally, the intensity of the beam, we should have to place our photometer considerably beyond the point x in order to employ the law of inverse squares with accuracy. Anywhere between the reflecting or refracting system, and

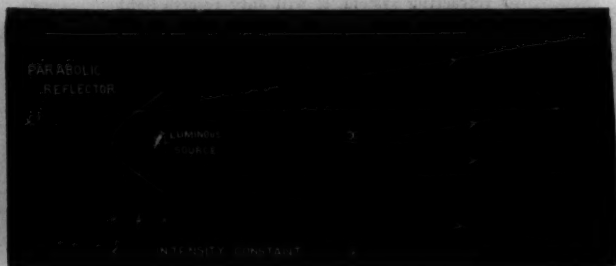


FIG. 7.

the point x , the illumination on the photometer screen will be constant, for from equation (3) we have (fig. 8),

Quantity of light falling on photometer screen P_1 is given by

$$\phi_1 = B \frac{A_1 P_1}{x_1^2}, \text{ and} \quad [A = \text{Area}]$$

quantity of light falling on photometer screen P_2 is given by,

$$\phi_2 = B \frac{A_2 P_2}{x_2^2}.$$

Now, if $x_2 = 2x$, then, owing to increased distance, illumination is one quarter, according to the law of inverse squares, but if the diameter of aperture $a_2 = 2a_1$ then the area of the aperture A_2 , will equal four times the area of the aperture A_1 . Consequently, the illumination on P_2 will equal the illumination on P_1 , providing a_2 does not exceed half the aperture of the reflecting or refracting system.

In other words, because the photometric screen is small (and is enclosed in a box usually) in comparison with the aperture of the reflecting or refracting system, it will only receive light from a certain definite area of the mirror or lens surface when it occupies any position inside the point x . The area of the active surface will be proportional to the square of the distance of the photometric screen from the surface

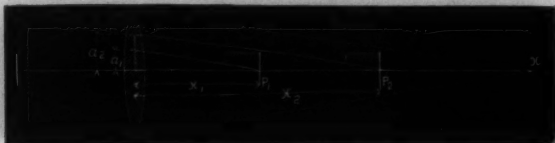


FIG. 8.

(mirror or lens), so that up to the point x the illumination on the photometer screen will be constant. Some objection might reasonably (at first sight) be raised to the idea of applying the law of inverse squares for light between the mirror or lens surface and the point x , but the experimental results of M. Fèry definitely show that we can, with accuracy, and, therefore, that the reason why the illumination remains constant up to the point x is because $\frac{\text{active surface}}{\text{distance squared}}$ remains constant.

M. Fèry obtained the following results with a lens 7 cm. in diameter and of 78 cm. focal

length. It was sensibly deprived of chromatic and spherical aberration for parallel rays in the direction of the axis. He attributes the constant difference between the experimental and calculated values to reflection from the four faces of the two lenses constituting the achromatic objective.

Distance of Photometer from source of Light.	Illumination observed—Candle-meters.	Illumination calculated—Candle-meters.
78 cm.	3.30	3.70
120 "	3.19	3.70
178 "	3.30	3.70
278 "	3.25	3.70
378 "	3.19	3.70
428 "	3.30	3.70
528 "	1.88	2.76
578 "	1.56	2.24
678 "	1.18	1.57
778 "	0.735	1.15
1,078 "	0.255	0.56
1,578 "	0.111	0.24



FIG. 9.

M. Fèry employed, as a luminous source, an aperture 1.4 cm. diameter illuminated by a Bengel jet, when making the above observations.

The calculated values were obtained by M. Fèry in the following manner:—

From the above diagram (similar to the one

given by him) we have $\tan \alpha$ or $\alpha = d/f = D/x$.

$$\therefore x = \frac{Df}{d} = \frac{7 \times 78}{1.4} = 39 \text{ meters for the}$$

case in hand.

At the point x the lens appears uniformly illuminated, and may be considered as a true source of light when viewed along the optic axis. Outside the point x the illumination varies according to the ordinary law, viz., inversely as the square of the distance. Obviously, then, since the illumination between the lens and the point x is constant, the multiplying power of the system is x^2 . The reflecting or refracting system may, consequently, be said to pass the light through a distance x , without any loss in illu-



FIG. 10. (THE HORIZONTAL MEASUREMENTS
PRESENT DISTANCE FROM SOURCE IN METERS.)

mination per unit area taking place upon the receiving screen.

The illumination per unit area of the lens surface in the above case is given by,

$$I = \frac{d \times B}{f^2}$$

where d represents the area of the luminous source in square mm., and B the brilliancy per unit area, or number of candles per square mm., of luminous surface. This, therefore, will give the illumination at the point x too.

M. Fèry's results are shown graphically in the following diagram, which is reproduced from his article (fig. 10).

The first curve refers to the naked arc, the second to the observed results given above, and the third to the calculated results given above.

M. Fèry refers to a case in which the factor x^2 was wrongly applied.

It was at Chicago, the projector under consideration consisted of a lens of 1 meter diameter and 1.5 meters focal length, illuminated by an arc crater of 20 square mm. area. The value of x^2 in this case would, therefore, be 90,000 ($x=300$ meters). If the illumination at the point x , due to the naked arc, is obtained, then the product of this result and the factor (x^2) will give the illumination due to the projector at the point x . At Chicago they obtained their result, 270 million candles, by employing the factor x^2 while obtaining the illumination due to the naked arc at a distance of 1 meter! As M. Fèry points out, if the photometer were placed quite close to the lens, an illumination of 1,333 candle-meters should be obtained with the above conditions.

$$\text{For } I = \frac{K}{d^2} = \frac{20 \times 150}{(1.5)^2} \text{ candle-meters, taking}$$

a value of 150 candles of intensity per square mm. of crater. This illumination will be maintained up to a distance of 300 meters from the lens in this case.

REPORT OF THE OPTICAL STANDARDS COMMITTEE ON THE STANDARDIZATION OF TRIAL CASES.

*Adopted by the Optical Society, June 18th,
1908.*

THE Trial Case Sub-Committee, with the help of the National Physical Laboratory, has devoted much consideration to the question of the standardization and certification of cases of trial lenses. The scheme proposed for adoption is detailed in this report.

Reference will be made to the Recommendations of the Optical Standards Committee as amended and adopted by the Optical Society on May 16, 1904. The principal points to be considered in connection with trial cases are dealt with in the order in which they are set forth in the "Recommendations," viz.:—

- (1) The powers of spherical and cylindrical lenses.
- (2) The marking of axes of cylindrical lenses and of the base apex lines of prisms.
- (3) The powers of prisms.
- (4) The centering of spherical and cylindrical lenses.
- (5) The transmission of tinted glasses.

The following tables and curves are given in an Appendix to this Report:—

Table of powers, curvatures, etc., of positives—(a) of minimum thickness, (b) of standard thickness—which will neutralize the standard negatives from 20 D to 10 D.

Table of curvatures of standard negatives.

Complete table of limits of error permitted.
Curves showing the transmission throughout the spectrum of (a) a neutral-tinted, (b) a blue glass, each having a mean transmission value in the neighbourhood of 30 per cent.

(1) The Powers of Spherical and Cylindrical Lenses.

In dealing with the standardization of powers, the Sub-Committee was met by a difficulty which has been the subject of much discussion. It is the common practice to test the powers of positive and negative lenses against one another by neutralization. For the low-power lenses this method is entirely satisfactory; but in the high-power lenses, by reason of their thickness, the true power of the positive is lower than that of the negative which it neutralizes. Thus the power of the positive lens which will neutralize a -20 D is approximately +19 D. A table is annexed to this Report which shows, for certain thicknesses of the positive lenses, the true powers of the positives which will neutralize the negatives ordinarily supplied in trial cases.

It was necessary, then, to decide what system should be adopted in the standardization of the high-power lenses. The matter was discussed in October, 1907, at a meeting of manufacturers and others interested, and there was a great preponderance of opinion in favour of standardization on what may be called a neutralization basis. This view has been accepted by your Committee, and in accordance with it the following recommendations are made:—

1. That the neutralization method of testing for power be accepted as the basis for the scheme of standardization to be adopted.

2. That the true powers of the negative lenses be required to agree with the values marked on the lenses, or their mounts. The limits of error allowed to be $\pm 2\%$, or ± 0.01 D where this is greater than 2% .

3. That the standard thicknesses and powers for the positive lenses be such that the positives will neutralize the corresponding negatives. That the *nominal* values given to these standard powers be the same as those of the negatives they neutralize. That the standard thicknesses and powers for lenses of powers not less than 10 D be those given in Table I.; the standard thickness for the corresponding negative, at the centre, being taken to be 1 mm.

4. That the limits of error allowed from the aforesaid standard powers, in the case of positive lenses of powers not less than 10 D, be $+2\frac{1}{2}\%$ from 20 D to 13 D inclusive, and $+3\%$ from 12 D to 10 D inclusive; and -1% from 20 D to 10 D inclusive.

5. That for the powers of positive lenses of nominal value below 10 D, the limits of error allowed from the *nominal* values be $\pm 2\%$, or ± 0.01 D where this is greater than 2% , as for the corresponding negatives.

TABLE I.
Shewing Thicknesses and True Powers of Standard Positives.

NOMINAL POWER.		STANDARD THICKNESS. (At centre.)		TRUE POWER.*
Diopters.		cm.		Diopters.
+ 20	...	.78	...	18.84
18	...	.71	...	17.13
16	...	.64	...	15.38
15	...	.60	...	14.48
14	...	.57	...	13.57
13	...	.53	...	12.65
12	...	.49	...	11.72
11	...	.46	...	10.78
10	...	.43	...	9.83

* The values given in this table are correct, within .02 diopter, for any value of the refractive index between 1.50 and 1.54. (See Appendix, Table III.)

6. That a statement, giving the true powers of the standard positives, be required to be pasted in each trial case sent for certification. The form of such statement is given at the end of this report.

The following explanations are given of certain points in connection with the foregoing recommendations.

The standard thicknesses given in Table I. are based on the minimum thicknesses possible for a diameter of 3.8 cms. ($1\frac{1}{2}$ ") and a refractive index of 1.50, with an allowance for edge thickness of about half a millimeter; this gives results in close agreement with the average value of observed thicknesses. It should be noted that somewhat wide variations in thickness will produce but little effect on the power: the subjoined table shows the change of power due to an increase of 1 mm. in thickness, the curvatures of the surfaces of the lens being supposed to remain unaltered.

TABLE II.

Power (Nominal).	Change due to an increase in thickness of 1 mm.			
20 D	...	...	...	-.06
18	...	...	...	-.05
16	...	...	...	-.04
15	...	...	...	-.04
14	...	...	...	-.03
13	...	...	...	-.03
12	...	...	...	-.02
11	...	...	...	-.02
10	...	...	...	-.02

It will be seen from Table III. that the power of the standard positive which will neutralize a given negative is practically independent of the refractive index for values within the ordinary range for trial case lenses, viz., between 1.50 and 1.54, provided the positive be made of the standard thickness. Even for wide variations

in the refractive index, the change in the power is small. Further, if the value of the index be increased to 1.6, and the thickness reduced to the minimum for this power, the power of the positive which neutralizes the standard negative will still be well within the limits of error allowed. These limits will thus include any equi-convex lens which will neutralize the corresponding standard negative.

With regard to the proposed limits of error to be allowed, it will be seen that in extreme cases the neutralization of a positive and a negative lens of the same nominal power may be only approximate. In no case, however, can the departure of the power from the mean value laid down by this scheme exceed 2% (or 0.01 D if greater than 2%), and it can hardly be doubted but that this degree of accuracy is sufficient for all practical purposes for which the trial case is employed in ophthalmic practice.

With reference to the proposed limits of error for the high-power positives, it is found that in practice the powers are generally higher than those set down as standard; and it is thought desirable that the power should not be allowed to fall appreciably below the standard value. Hence different + and - limits of error are allowed.

It may be remarked here that, when required by the manufacturer, standard lenses can be measured at the National Physical Laboratory to a high degree of accuracy, which is practically limited only by the accuracy to which their surfaces are ground.

(2) The Marking of Axes of Cylindrical Lenses and of the Base-Apex Lines of Prisms.

The only point requiring consideration under this head is the limit of error to be allowed. Your Committee recommends that this limit be 2° throughout.

In fixing this value it must be borne in mind

that for a lens of 3.8 cm. ($1\frac{1}{2}$ ") diameter a displacement of each of the marks determining the axis by 0.3 of a mm. introduces an error of 1° .

It is a common practice to indicate the base-apex line of a prism by one mark only. This is unsatisfactory from all points of view, and for certification purposes two marks will be required. When the prism is mounted so that one face is flush with the rim, the markings should preferably be on this face.

(3) The Powers of Prisms.

The power of a prism is measured by the minimum deviation produced by it, and this deviation is expressed by the number of centimeters displacement produced in an object at one meter distance; the unit is called the prism diopter (Δ).

Your Committee recommends that the following limits of error be allowed for prisms:—

POWER.			LIMIT OF ERROR.	
Δ				
1 to 3	...	...	$\pm$	.10 Δ
4 to 8	...	...	$\pm$	.20 Δ
10 to 12	...	..	$\pm$	.30 Δ
15	...	...	$\pm$	.40 Δ
20	...	...	$\pm$	.50 Δ

In regard to the high-power prisms, the present practice as regards the measurement of the power is somewhat unsettled. Instead of the minimum deviation being measured, in some cases the deviation when the light is normal to the prism face at the point of incidence, or of emergence, is observed.* The difference is negligible except for prisms of powers 10 Δ or more, which are very seldom used.

* NOTE.—It may be remarked that if, in the measurement of the power, the light incident on, or emergent from, one face is normal to the other—a condition easily realized in practice—the deviation is very nearly a minimum.

Your Committee recommends:—

That for standardization purposes the minimum deviation be taken as determining the power; but that in cases where a different practice has been followed the prisms might be passed, with a statement indicating the divergence from the standard practice.

(4) The Centering of Spherical and Cylindrical Lenses.

The following limits of permissible error are recommended:—

POWER.	LIMITS OF ERROR mm.
From 20 D to 1 D inclusive ...	1
„ .87 D to .37 D „ ...	2
„ .25 D	3
„ .12 D	5

The last limit may seem large, but it corresponds to a prismatic effect of only .06 of a prism diopter, which is negligible in practice; and observations show that errors near this limit are fairly common in the best trial cases.

On the other hand, a limit of error of 1 mm. in the higher powers is of some practical importance, introducing as it does 1 Δ prism effect in a 10 D lens. Though it is not possible to fix a lower limit, every effort should be made by the manufacturer to keep the error below $\frac{1}{2}$ mm. in the higher powers.

(5) The Transmission of Tinted Glasses.

In the "Recommendations" of May 16, 1904, transmission values for a suitable series of standards for neutral-tinted and coloured glasses were laid down. An attempt has been made to realize a series of standards, both in neutral-tinted and blue glasses, having approximately the values assigned in the "Recommendations," as far at least as No. 8.

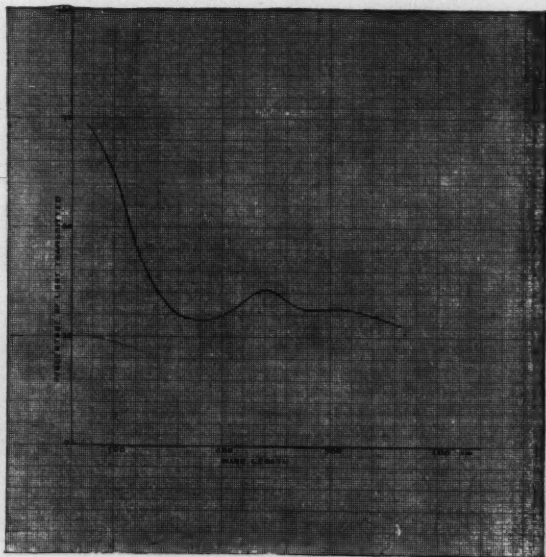
The following table gives the standard transmission values and the limits of error recommended.

OPTICAL SOCIETY'S STANDARD TRANSMISSION VALUE.					LIMITS OF ERROR.
No. 1	...	80%	...	...	$\pm 5\%$
" 2	...	65 "	...	...	$\pm 5 "$
" 3	...	50 "	...	...	$\pm 4 "$
" 4	...	40 "	...	...	$\pm 3 "$
" 5	..	30 "	...	...	$\pm 3 "$
" 6	...	20 "	...	...	$\pm 3 "$
" 7	...	10 "	...	...	$\pm 2 "$
" 8	...	5 "	...	...	$\pm 2 "$

A set of standard neutral-tinted glasses has been provisionally selected, and their transmission values, under certain definite conditions, have been determined at the National Physical Laboratory.

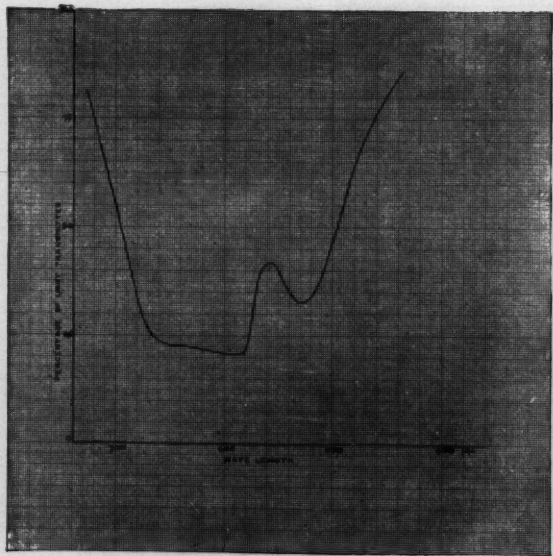
The source of light employed is a 20-volt 4-candle-power carbon filament glow lamp, giving light approximately of the same quality as the ordinary commercial glow-lamp. Such light differs in character from ordinary daylight, being weaker especially in the blue and violet rays, and the transmission values obtained, therefore, must be regarded as more or less arbitrary. The following reasons, however, may be urged in favour of the course adopted:

- (1) The source employed is very convenient for purposes of measurement.
- (2) In any measurements which may be made by manufacturers, an electric incandescent lamp, or a source of similar character, is more likely to be employed for photometric purposes than what is ordinarily understood by white light.
- (3) The results are readily reproducible, observations made by three different observers at long intervals of time being in satisfactory agreement. The



The National Physical Laboratory.

CURVE SHOWING THE PERCENTAGE TRANSMISSION
THROUGHOUT THE SPECTRUM OF A NEUTRAL-TINTED
GLASS HAVING A MEAN TRANSMISSION VALUE OF 35 PER
CENT, APPROXIMATELY



The National Physical Laboratory.

CURVE SHOWING THE PERCENTAGE TRANSMISSION
THROUGHOUT THE SPECTRUM OF A BLUE GLASS HAVING
A MEAN TRANSMISSION VALUE OF 34.5 PER CENT.
APPROXIMATELY.

observed values of the ratios were also in good agreement with the values directly obtained.

It is recommended, therefore—

That the values obtained as above explained be adopted and be called the transmission values of the standard glasses.

The transmission values of other glasses can be obtained in terms of these standards to a high degree of accuracy.

The transmission values of the present provisional set of neutral-tinted standards are as follows:—

No. 1	...	...	...	78.9
" 2	...	...	...	66.6
" 3	...	...	...	52.0
" 4	...	...	...	39.3
" 5	...	...	...	29.8
" 6	...	...	...	19.7
" 7	...	...	...	10.3
" 8	...	...	...	6.1

With one exception, these glasses were furnished by Mr. W. A. Dixey, who has kindly offered to supply other glasses, with a view to approximating still more closely to the standard values suggested in the "Recommendations" of the Optical Standards Committee. The values of any new glasses will be, of course, expressible in terms of the values of the provisional set. It is hoped that a uniform and suitable series of standard neutral-tinted glasses may thus be secured.

A similar procedure has been followed with regard to standard blue glasses, the measurements in this case being made with a flicker photometer. Provisional standards have been selected, and the values of other glasses will be expressed in terms of the more or less arbitrary values assigned to these standards. For the immediate purpose under consideration the blue glasses are regarded as relatively less important.

It has been thought of interest to add to this Report curves showing the percentage transmission throughout the spectrum of a neutral-tinted and a blue glass respectively, having mean transmission values in the neighbourhood of 30%.

Notice to be pasted or fixed in each trial case submitted for certification.

In this Trial Case the High-Power Positive Lenses are made to neutralize the corresponding negatives, and, therefore, their true powers differ somewhat from their nominal powers as marked on the lenses. The following table gives the true powers of Standard Neutralizing Lenses, as fixed by the National Physical Laboratory:

NOMINAL POWER.	TRUE POWER.
Diopters.	Diopters.
+20	18.84
18	17.18
16	15.38
15	14.48
14	13.57
13	12.65
12	11.72
11	10.78
10	9.88

The positive lenses below 10 D, and the negative lenses throughout, are made to the values marked on the lenses.
mounts.

Signed on behalf of the Committee,

R. MULLINEUX WALMSLEY,
Vice-Chairman.

APPENDIX.—
TABLE III. POWERS OF POSITIVES TO NEUTRALIZE STANDARD NEGATIVES, FOR DIFFERENT VALUES OF THE REFRACTIVE INDEX n_D : (1) when positive is of minimum thickness (t) for the diameter of $1\frac{1}{2}$ inches; (2) when positive is of suggested standard thickness. The radius of curvature of surface (r), the focal length (f), and the distance ($f.d.$) of the focus from the nearest point of the surface (for standard thickness), are also given.

D NOMINAL	MINIMUM THICKNESS.					STANDARD THICKNESS.				
	n_D	t cm.	r cm.	f cm.	D	t cm.	r cm.	f cm.	$f.d.$ cm.	D
20 ...	1.50	.727	5.168	5.292	18.90	.780	5.179	5.312	5.044	18.83
	1.51	.714	5.268	5.286	18.92	"	5.282	5.310	"	18.83
	1.52	.700	5.368	5.279	18.94	"	5.384	5.308	"	18.84
	1.53	.687	5.467	5.272	18.97	"	5.486	5.306	"	18.85
	1.54	.673	5.566	5.265	18.99	"	5.587	5.303	"	18.86
18 ...	1.50	.654	5.709	5.820	17.18	.710	5.719	5.840	5.598	17.12
	1.51	.642	5.818	5.814	17.20	"	5.833	5.838	"	17.13
	1.52	.630	5.928	5.808	17.22	"	5.946	5.836	"	17.14
	1.53	.617	6.040	5.802	17.23	"	6.059	5.834	"	17.15
	1.54	.605	6.153	5.797	17.25	"	6.172	5.832	"	17.15
16 ...	1.50	.582	6.387	6.486	15.42	.640	6.397	6.506	6.288	15.37
	1.51	.571	6.512	6.480	15.43	"	6.525	6.505	"	15.37
	1.52	.559	6.637	6.475	15.44	"	6.652	6.503	"	15.38
	1.53	.548	6.762	6.469	15.45	"	6.779	6.501	"	15.38
	1.54	.537	6.886	6.464	15.47	"	6.905	6.499	"	15.39
15 ...	1.50	.545	6.797	6.889	14.52	.600	6.807	6.908	6.705	14.48
	1.51	.534	6.931	6.885	14.53	"	6.942	6.906	"	14.48
	1.52	.523	7.065	6.880	14.53	"	7.077	6.905	"	14.48
	1.53	.513	7.197	6.875	14.54	"	7.212	6.903	"	14.49
	1.54	.504	7.329	6.869	14.56	"	7.347	6.902	"	14.49

14 ...	1.50	.509	7.265	7.351	13.60	.570	7.276	7.372	7.179	13.56
	1.51	.499	7.408	7.347	13.61	"	7.421	7.370	"	13.57
	1.52	.489	7.551	7.342	13.62	"	7.565	7.369	"	13.57
	1.53	.479	7.693	7.337	13.63	"	7.710	7.368	"	13.57
	1.54	.470	7.835	7.332	13.64	"	7.854	7.367	"	13.57
13 ...	1.50	.472	7.807	7.887	12.68	.580	7.818	7.907	7.728	12.65
	1.51	.462	7.962	7.883	12.69	"	7.978	7.905	"	12.65
	1.52	.453	8.116	7.879	12.69	"	8.128	7.904	"	12.65
	1.53	.445	8.269	7.875	12.70	"	8.288	7.902	"	12.66
	1.54	.437	8.422	7.870	12.71	"	8.439	7.901	"	12.66
12 ...	1.50	.435	8.442	8.515	11.74	.490	8.452	8.534	8.369	11.72
	1.51	.426	8.610	8.511	11.75	"	8.619	8.532	"	11.72
	1.52	.418	8.777	8.508	11.75	"	8.787	8.531	"	11.72
	1.53	.410	8.943	8.504	11.76	"	8.955	8.530	"	11.72
	1.54	.403	9.109	8.501	11.76	"	9.124	8.529	"	11.72
11 ...	1.50	.399	9.192	9.259	10.80	.460	9.203	9.281	9.125	10.77
	1.51	.391	9.375	9.256	10.80	"	9.386	9.279	"	10.78
	1.52	.384	9.557	9.253	10.81	"	9.569	9.278	"	10.78
	1.53	.376	9.738	9.249	10.81	"	9.753	9.277	"	10.78
	1.54	.369	9.919	9.245	10.82	"	9.937	9.276	"	10.78
10 ...	1.50	.363	10.095	10.156	9.85	.430	10.107	10.179	10.085	9.82
	1.51	.356	10.295	10.153	9.85	"	10.308	10.178	"	9.83
	1.52	.349	10.496	10.150	9.85	"	10.510	10.177	"	9.83
	1.53	.342	10.696	10.146	9.86	"	10.711	10.176	"	9.83
	1.54	.336	10.896	10.143	9.86	"	10.913	10.175	"	9.83

TABLE IV. Table of Focal Lengths and Curvatures of Standard Bi-concave Negatives of thickness at the centre 1 mm.

D	n_D	f cms.	r cms.	D	n_D	f cms.	r cms.	D	n_D	f cms.	r cms.
20	{ ... }	5.000	5.017	15	{ ... }	1.50	6.667	12	{ ... }	1.50	8.333
		"	5.117			1.51	"			1.51	"
		"	5.217			1.52	"			1.52	"
		"	5.317			1.53	"			1.53	"
		"	5.418			1.54	"			1.54	"
18	{ ... }	5.555	5.572	14	{ ... }	1.50	7.143	11	{ ... }	1.50	9.091
		"	5.683			1.51	"			1.51	"
		"	5.794			1.52	"			1.52	"
		"	5.905			1.53	"			1.53	"
		"	6.017			1.54	"			1.54	"
16	{ ... }	6.250	6.267	13	{ ... }	1.50	7.692	10	{ ... }	1.50	10.000
		"	6.892			1.51	"			1.51	"
		"	6.517			1.52	"			1.52	"
		"	6.642			1.53	"			1.53	"
		"	6.767			1.54	"			1.54	"

TABLE V. Limits of Error permitted.

	RANGE (Inclusive).	STANDARD VALUE.	LIMITS OF ERROR.
Power, negative lenses (spherical and cylindrical)	- 20 D to - .50 D	as marked	$\pm 2\%$
	- .87 D to - .12 D	" "	$\pm .01$ D
Power, positive lenses ...	+ 20 D to + 18 D	as in Table I.	$+ 2\frac{1}{2}\%$ } or $- 1\%$
	+ 12 D to + 10 D	" "	$+ 3$ " }
	+ 9 D to + .50 D	as marked	± 2 "
	+ .87 D to + .12 D	" "	$\pm .01$ D
Axis, cylindricals ... Base-Apex line, prisms ...	...	...	2°
	...	...	$2q$
Power, prisms ...	20 Δ	as marked	$\pm .50 \Delta$
	15 Δ	" "	$\pm .40 \Delta$
	12 Δ to 10 Δ	" "	$\pm .30 \Delta$
	8 Δ to 4 Δ	" "	$\pm .20 \Delta$
	3 Δ to 1 Δ	" "	$\pm .10 \Delta$
Centering, spherical and cylindrical lenses ...	20 D to 1 D	...	1 mm.
	.87 D to .37 D	...	2 "
	.25 D	...	3 "
	.12 D	...	5 "
Neutral-tinted and blue glasses	Transmission Value—		Of the incident light --
	No. 1	80 %	$\pm 5\%$
	" 2	65 "	± 5 "
	" 3	50 "	± 4 "
	" 4	40 "	± 3 "
	" 5	30 "	± 3 "
	" 6	20 "	± 3 "
	" 7	10 "	± 2 "
	" 8	5 "	± 2 "
			± 2 "

SOME NEW OPTICAL PROJECTION APPARATUS.

BY MR. FREDERICK J. CHESHIRE.

Paper read before the Optical Society.

THE optical-projection apparatus which I propose to describe and demonstrate to you this evening consists in the main of polarising apparatus which has been designed in the interests of the teacher desirous of utilising polarisation phenomena for the purpose of teaching and demonstrating the nature of light and the mechanics of its propagation. Whilst for the subjective demonstration of these phenomena the necessity for that rare crystal—Iceland Spar—is, happily, reduced to a minimum, it is unfortunate that their objective demonstration, at any rate, to a considerable number of people, makes much more serious demands upon it. Iceland spar has never been obtained, I believe, of optical quality and of large size, except from a single mine in Iceland, and even this supply has now been exhausted for some years. The utilisation, then, to the best advantage of that spar which we do possess is now a question of paramount importance. Now, England, in the matter of polarising apparatus, has been the home of the big nicol—one at each end of the polariscope. Mr. Ahrens, for instance, our prince of polarising prism makers, who has been so closely identified with polarisation work in England for nearly 40 years, and whose prisms are known all over the world, tells me that commencing in 1870 with a pair of nicols of $2\frac{1}{2}$ inches aperture for the late Mr. Spottiswoode, he finally reached in 1873 an aperture of $3\frac{1}{8}$ inches, making several other prisms, about

this date, with apertures of $3\frac{1}{2}$ inches. No doubt, if the present scarcity of spar had been foreseen the problem of its economic use would have been more seriously taken in hand. This was not, however, done, with the result that the poor lecturer on physics of to-day—the child of the hard-hit gambler—is in want of the barest necessities for his table.

In France, the famous instrument maker and optician, Duboscq, with true appreciation of the value of the projection-polariscope as a scientific instrument for the teacher of optics, wisely designed apparatus for this purpose rather than for the use of the lecturer desirous of dazzling his audience with chromatic splendours.* Duboscq abandoned the nicol prism, therefore, as a polariser and adopted the double-image prism in its place, contenting himself with a small nicol prism as an analyser. But the substitution of a double-image prism for a nicol creates many experimental difficulties. In practice the nicol, no matter what its aperture or the size of the light source may be, transmits only one of the two beams of polarised light produced by double refraction; in the double-image prism on the other hand, both of these beams are transmitted, the beam not required being either stopped or deflected to one side to prevent it from reaching the screen. But for many reasons, chiefly on account of the size of the light-source and the spherical aberration of the lens system employed, this separation of the two beams is a difficult matter with a prism in which the angular separation of the two polarised beams does not exceed a few degrees, as it cannot very well do. In Duboscq's apparatus, when the double-image prism is worked at full aperture, the light from the electric arc is focussed through it upon a metal plate or dia-

* For a description of this apparatus see an article by Bertin, 'Journal de Physique,' Vol. IV., 1875.

phragm. One image of the source is adjusted so as to fall on a 5 mm. hole, which thus becomes practically a small source of diverging plane-polarised light, whilst the light forming the second image is stopped. This arrangement, no doubt, is fairly effective so long as a small and brilliant source of light, as the arc, is employed, but when a more extended source is employed it is obvious that the two images of the source must overlap more or less, with the result that the complete separation of the two polarised beams becomes difficult or impossible.

My work, therefore, has had for its object the improvement of the projection polariscope employing a double-image prism as a polariser. This object, I think, I have been enabled to effect in a marked degree by the simple expedient of first focussing the light-source, by means of the ordinary lantern condenser, upon a circular hole of from 1 cm. to 2 cm. in diameter in a metal plate and then using this sharply defined luminous disc as the virtual source for the polariscope. Following from this it has been possible to secure over the Duboscq type of apparatus the following advantages amongst others:—

1. Projection results with large prisms practically equal to those obtainable with nicols of the same aperture.
2. A simplification of the optical system required.
3. The use, with advantage, of a double-image prism as a polariser with a light-source of any size.
4. A great increase in the separation of the two components of the condensing system for a given angular aperture.

If the double-image prism is to replace the nicol prism, its *raison d'être* must be the more economical use of spar. The method of cutting the spar prisms for such double-image prisms from the natural cleavage rhombohedron becomes, therefore, a matter of first-class import-

ance. To this problem Mr. Ahrens has applied himself with his usual success.

He proposes to take a rhombohedron of spar AB of such a length that a line drawn from one obtuse angle to the other bisects these angles in the plane of symmetry as shown. After dividing the spar along this line in a plane normal to the paper each of the halves can be made into a double image prism by making them direct-

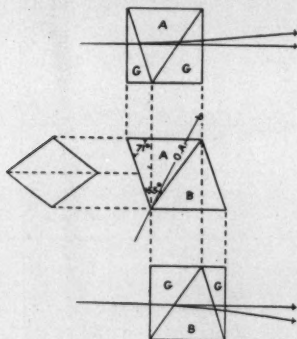


FIG. 1.—AHREN'S DOUBLE-IMAGE PRISM.

G. I have had a pair of prisms cut in this way with apertures of 50 mm., which are entirely successful. They give a separation of about 8° .

POLARISCOPE (PARALLEL LIGHT).—The fittings carried by saddle stands arranged to clamp and slide on a triangular bar as in Zeiss' microscope-projection apparatus, are as follows:—

- D. A diaphragm in the plane of which the lantern (not shown) is arranged to focus the arc in a circular aperture of from 1 cm. to 2 cm. diameter.
- C. Collimating lens of about 25 cm. focus.

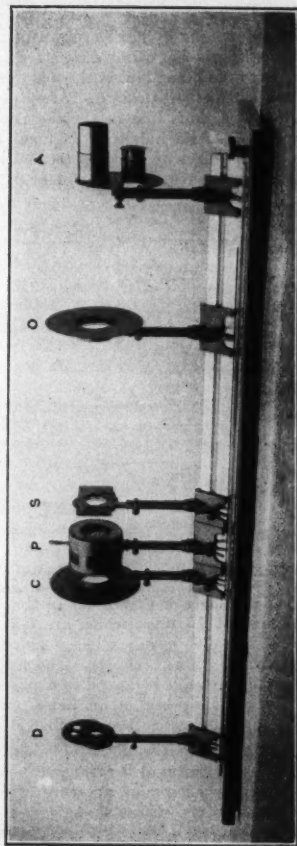


FIG. 2.—POLARISCOPE ARRANGED FOR EXPERIMENTS IN PARALLEL PLANE-POLARISED LIGHT.

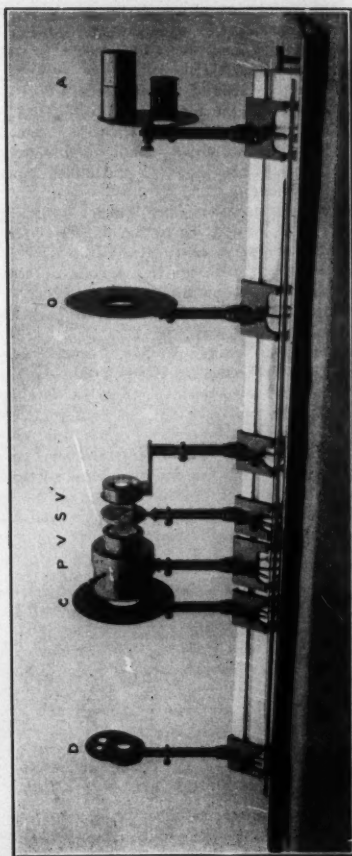


FIG. 3.—POLARISCOPE ARRANGED FOR EXPERIMENTS IN CONVERGENT PLANE-POLARISED LIGHT.

P. The polariser—a double-image prism of about 5.2 cm. aperture and giving a separation of about 8° .

S. Object holder.

O. Projecting lens of 25 cm. focus.

A. Analyser in the form of a nicol prism of 2.5 cm. aperture mounted in a rotatable disc in which a small double-image prism is also mounted and a clear aperture made.

POLARISCOPE (CONVERGENT LIGHT).—Fittings D, C, P, S, O, and A, as in fig. 2. The prism holder P carries a converging system V of about 5 cm. aperture, giving a cone of from 70° to 75° . A second and similar system, V¹ parallelises the light after passing through the object.

[The apparatus shown in figs. 2 and 3 were used for the projection of the polarisation patterns produced by such objects as selenite designs, axis-cut quartz in various combinations, unannealed and screw-stressed glass, the ring systems of uni- and bi-axial crystals, etc., etc.]

The way in which the deflected beam of light is thrown out of the optical systems shown by figs. 2 and 3, respectively, is shown by the following diagrams.

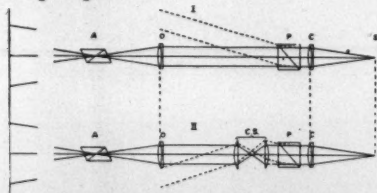


FIG. 4.—RAY DIAGRAM FOR POLARISCOPE WITH DOUBLE-IMAGE PRISM AS POLARISER USING (I) PARALLEL AND (II) CONVERGENT LIGHT.

SPECTRO-POLARISCOPE.—The apparatus already described may be modified in a beautifully simple way to throw upon the screen two

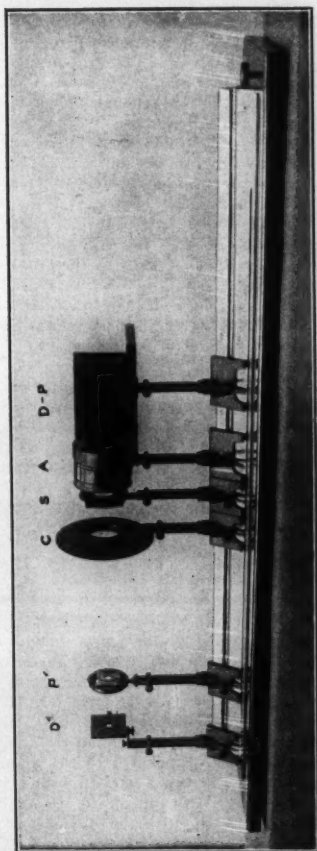


FIG. 5.—SPECTRO-POLARISCOPE.

vertically superposed spectra, in one of which the light is polarised in a vertical plane, and in the other in a horizontal plane, these spectra remaining *stationary* during the rotation of the analyser, which may be a double-image prism.

The fittings are as follows:—

D¹. A slit of adjustable width upon which the lantern focusses the arc.

P¹. A small double-image prism of from 15 to 18 mm. square, and giving a deviation of from 3° to 4° interposed between the slit D¹ and the collimating lens C, to present virtually to the latter two superposed slits, in one of which the light is polarised in a vertical plane, and in the other in a horizontal plane.

C. Collimating and projecting lens.

S. Object holder.

A. Analyser in the form of a nicol prism, or it may be a double-image prism. In the latter case a screen must be placed near the end of the bench to cut off from the wall screen two of the four spectra produced.

D-P. Direct-vision cinnamic-ether or other spectrum giving prism.

In the absence of the fittings beyond the collimating lens C two superposed slits are projected upon the screen in which the light is oppositely polarised as explained. The interposition of the prism D-P expands these images into spectra. Thus, when the analyser A—a nicol prism, say, for simplicity—is used, as shown, and rotated, the spectra on the screen are alternately cut out and restored according as to whether the plane of polarisation of the analyser is at right angles or parallel to the plane of polarisation of the two polarised beams from the virtual slits. When, therefore, the analyser is so set as to extinguish the upper spectrum and a plate of axis-cut quartz is placed in the holder S, the upper spectrum reappears together

with the lower one. Each spectrum is then seen to contain a number of dark absorption bands, each band in one spectrum being opposite to a bright band in the other, *i.e.*, the spectra are complementary to one another. Now, if the analyser be rotated clockwise as seen with the back to the screen the bands will travel from the red to the violet ends of the spectra, or *vice-versâ*, according as the quartz under examination is right- or left-handed, respectively. Similar bands, which do not, however, travel, can be obtained from films of mica, selenite, etc. One important advantage and novelty of the apparatus described is that the spectra remain stationary during the rotation of the analyser. By moving the prism P¹ along the base bar the spectra can be caused to separate or approach vertically. By causing them to overlap a middle spectrum of natural light is obtained, in which no absorption bands appear.

PROJECTION-APPARATUS FOR DEMONSTRATING THE
OPTICAL ACTION OF THE EYE AND ITS OPTI-
CAL DEFECTS.

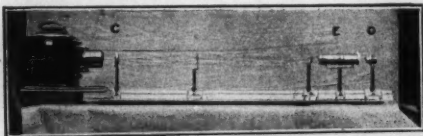


FIG. 6.—APPARATUS FOR PROJECTING MODEL-EYE
EXPERIMENTS.

This apparatus is shown ready for use in fig. 6. It consists of three parts, *viz.*, (1) the model eye E (to be afterwards described) with a lens O to throw an image of the "retina" upon the screen, both of which are mounted on a half-meter bar; (2) a meter bar placed between the model eye and the lantern shown, upon which near-point experiments are made; (3) a

lantern employed to give the light necessary for projection, and also for giving an object at infinity in the shape of a slide in the ordinary carrier at the focus of a lens C, which can be thrown into and out of action laterally.

The model eye E consists of a tube about 25 cm. long with an achromatic lens ($f=25$ cm.) and iris diaphragm at one end and adapted to receive at the other either one of three mounted screens, or "retinas," which, when pushed home in the tube, are at the required distances from the lens to represent (1) the normal eye, (2) the

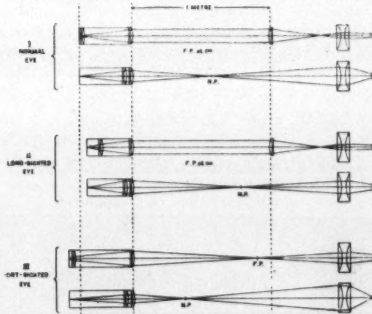


FIG. 7.—MODEL-EYE RAY DIAGRAMS SHOWING THE FAR AND NEAR-POINT ADJUSTMENTS FOR (I) THE NORMAL EYE; (II) THE LONG-SIGHTED EYE; AND (III) THE SHORT-SIGHTED EYE.

long-sighted eye, and (3) the short-sighted eye. Between the fixed lens and the retina a second achromatic lens is adapted to slide to give to the eye different powers of accommodation. When close up to the front lens the total refracting power of the eye is, of course, a maximum, whilst in the other extreme position, close up against the retina, the power of the sliding lens

is practically nothing, with the result that the front lens alone is effective. Between these two extreme positions intermediate powers are obtained. Behind the eye E, the lens O is adjusted to project on to an ordinary lantern screen, the retina, and any image which may be in focus upon it.

NORMAL EYE. FIG. 7 (I.).—The normal retina being fitted, and the accommodation lens being thrown out of action, any lantern slide collimated by the lens C, will be found imaged upon

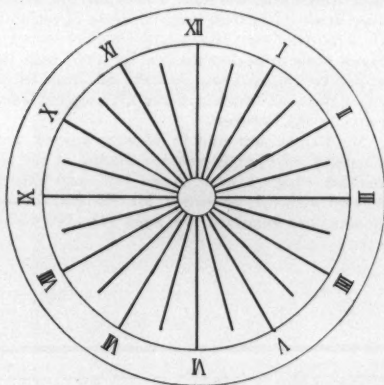


FIG. 8.—DIAGRAM FOR TESTING ASTIGMATISM.

the retina, and hence upon the lantern screen. By putting the accommodation lens in action the near-point (N.P.) can be determined and measured.

LONG-SIGHTED EYE. FIG. 7 (II.).—The proper retina having been fitted it will be found that an object at infinity (lantern slide at the focus of the lens C) cannot be focussed without calling upon the accommodation power by sliding upon the accommodation lens, as shown. A

determination of the near-point shows it to be at a greater distance than in the case of the normal eye.

SHORT-SIGHTED EYE. FIG. 7 (III.).—In this case both the far-point and the near-point are found to be within one meter of the eye.

In each of the above cases, the defects having been demonstrated, they can then be corrected by the use of spectacle lenses, and the corrected eye shown to be practically normal. By placing a diagram, such as shown by fig. 8, in the lantern, and fitting the fixed lens of the eye with a very weak cylindrical lens, the facts of astigmatism (and its correction) may be demonstrated. Many other experiments can be made with the apparatus which space forbids me from entering upon, but which will readily suggest themselves to the lecturer.

My thanks are due to Messrs. Newton and Company, of Fleet Street, for the use of the apparatus which I have shown you, and experimented with this evening. Mr. Wright, in particular, has rendered me most valuable assistance.

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